THURSDAY, SEPTEMBER, 2, 1880

THE CRUISE OF THE "KNIGHT ERRANT"

IT was accepted by us as one of the general conclusions from the temperature observations made on board the *Challenger* that the normal vertical arrangement of temperature in the ocean is somewhat in this wise. The water is warmest at the surface; from the surface it cools rapidly for the first hundred fathoms or so; it then cools more slowly down to five or six hundred fathoms; and then extremely slowly to the bottom, where the minimum temperature is reached.

I need not here enter into detail as to the causes of this normal condition, which have already been fully discussed.[1] I may state however, generally, that the temperature of the upper strata is raised by solar radiation, and its distribution is affected by currents and by many other local causes; and that the water which has been cooled down in the polar seas until it has acquired a high specific gravity, flows along the bottom and into the deepest abysses to which it has access.

This normal vertical distribution of temperature is by no means universal or even general; it exists only in those parts of the ocean which are continuous throughout their entire depth with a polar sea. No ocean is thus continuous with the Arctic Sea; a wide belt apparently under these normal temperature conditions surrounds the South Pole or the south polar land nearly if not entirely, but the gulf-like northward extensions of the water-hemisphere, the Atlantic and the Pacific, show a distribution of temperature to a certain extent abnormal, and in some seas which occupy more restricted areas, the deviation from the normal conditions is excessive. In oceans where the thermometer sinks steadily from the surface to the bottom, that is to say, in those under normal conditions, the bottom temperature at anywhere near 2,500 fathoms is a little below the freezing point. The Atlantic Ocean is divided into three areas; in one of these, an area extending from the Antarctic Sea along the coast of South America to ten degrees or so north of the Equator, the temperature sinks at the usual rate to $31^{\circ}\cdot5$ F. at the bottom (2,900 fathoms). In another, the eastern basin, extending along the coasts of Europe and Africa, the temperature sinks steadily to $35^{\circ}\cdot5$ at a depth of about 2,000 fathoms, and this temperature extends to the bottom (3,150 fathoms); in the third area, the western basin, off the West Indies and the coast of North America, the temperature falls to 35° at 2,000 fathoms, and this temperature is again continuous to the bottom (3,475 fathoms). As extreme instances of this abnormal condition, in the Celebes Sea, which attains a depth of 2,600 fathoms, the minimum temperature—$38^{\circ}\cdot5$ F.—is reached between 700 and 800 fathoms; the Banda Sea, with a depth of 2,800 fathoms, reaches its minimum temperature of 37° F. at 900 fathoms; and the Sulu Sea, which is at least 2,550 fathoms deep, has a uniform temperature of $50^{\circ}\cdot5$ F. from a depth of 400 fathoms to the bottom.

[1] "Hydrographic Proceedings of the Voyage of H.M.S. *Challenger*." Report on Temperatures by Staff-Commander Tizard, R.N. (London, 1876); "The Atlantic," by Sir C. Wyville Thomson, F.R.S., vol. ii. p. 300, *et seq.* (London: Macmillan and Co., 1877.)

The combined results of our soundings and serial temperature determinations led us to conclude that those ocean basins in which the water is of a uniform temperature from a certain depth to the bottom are inclosed within a continuous barrier of a height corresponding to the depth at which the fall in temperature ceases; and that consequently no water at a temperature lower than the isotherm of that depth can pass into them. Suppose such a barrier to rise, as it does rise, in the Atlantic between the south-western and the eastern basins to a height of 2,000 fathoms below the surface, a sounding on the west side to the depth of 2,500 fathoms close to the barrier would give a temperature a little below 32° F., while the thermometer at the same depth on the other side of the barrier would register $35^{\circ}\cdot5$ F. In this way we may have very different temperatures at the same depth, close to one another and apparently under absolutely similar circumstances, and from our experience we should be inclined to accept the existence of continuous barriers as the almost universal explanation of such phenomena.

Of course any generalisation such as I have indicated partakes more or less of the character of a speculation. It is impossible to trace out the entire line of the barrier limiting an ocean basin and to prove its continuity.

In discussing this matter during the cruise of the *Challenger*, Staff-Commander Tizard and I had often in our minds the singular instance of contiguous areas of widely different temperature conditions which had been examined by Dr. Carpenter and myself in the *Lightning* and the *Porcupine* in the years 1868 and 1869.

In the channel between the north coast of Scotland and the Shetland Islands, and the banks and islands of the Faroe group, the average maximum depth is from 500 to 600 fathoms. An abrupt line of demarcation divides this channel into two portions, one of which my colleague Dr. Carpenter called the *cold* and the other the *warm* area.[1] The temperature of the water to a depth of 200 fathoms is much the same in the two areas; in the *cold area*, which occupies nearly the whole of the channel, extending in a north-easterly direction from a line joining Cape Wrath and the Faroe fishing banks, the temperature at 250 fathoms is 34° F., and $30^{\circ}\cdot5$ at the bottom (640 fathoms); in the *warm area* which stretches south-westwards from the same line, the thermometer registers 47° F. at 250 fathoms, and 42° F. at the bottom (600 fathoms).

When the phenomenon was first observed, we concluded than an indraught of cold water, passing southwards from the Spitzbergen Sea, welled into the Faroe Channel, and was met at its mouth and banked in by the north-easterly extension of the Gulf Stream, forming along the line of contact and partial mixture a "cold wall," comparable with that described as occurring in the Strait of Florida between the cold water of the Labrador Current and the Gulf Stream near its origin. This view however presented many difficulties, and on reconsidering the matter

[1] "Preliminary Report by Dr. W. B. Carpenter, V.P.R.S., of Dredging Operations in the Seas to the North of the British Islands, carried on in H.M. steam-vessel *Lightning*," by Dr. Carpenter and Dr. Wyville Thomson, Professor of Natural History in Queen's College, Belfast (*Proceedings* of the Royal Society of London, vol. xvii.). "Preliminary Report of the Scientific Exploration of the Deep Sea in H.M. Surveying-vessel *Porcupine* during the Summer of 1869, conducted by Dr. Carpenter, V.P.R.S., Mr. J. Gwyn Jeffreys, F.R.S., and Prof. Wyville Thomson, LL.D., F.R.S. (*Proceedings* of the Royal Society of London, vol. xviii.)

it now seemed certain that if our generalisation with regard to the cause of great differences in bottom temperatures within short distances be correct, a submarine ridge rising to within about 200 fathoms of the surface must extend across the mouth of the channel between the coast of Scotland and the Faroe banks. We recognised this as a test case which we might probably be able to examine thoroughly, as it was within our easy reach and on a sufficiently small scale; and I determined to take the first opportunity of making a careful survey of the channel with Capt. Tizard's co-operation, if possible before the *Challenger* temperature results were finally discussed.

I was prevented by various circumstances from taking any active steps in this direction until last year, when the Hydrographer of the Admiralty kindly consented to arrange another opportunity for sounding the Faroe Channel. I was obliged again to postpone the undertaking on account of a severe illness, and it was not until the early part of the present summer that I felt well enough to renew my application. I then wrote the following letter to the Hydrographer:—

"*Bonsyde, Linlithgow, June* 16, 1880

"DEAR CAPTAIN EVANS,—As you are aware, during our cruise in H.M.S. *Lightning*, in the year 1868, Dr. Carpenter and I found to our surprise that the channel between the Faroe Island and the coast of Scotland consisted of two very distinct 'areas,' the deep water in the two divisions differing in temperature to a marked degree. Consequent upon the difference of temperature, the faunæ of the two areas were also different. The 'warm' area was separated from the 'cold' by a distinct line of demarcation running apparently from about Cape Wrath past the Island of Rona, and as far as the southern Faroe fishing banks. During the voyage of the *Challenger* we met on many occasions with an abrupt change in the deeper temperatures along a definite line, and we arrived at the general conclusion that the phenomenon depended in all cases upon the interruption of the flow of an undercurrent by a raised submarine ridge. The instance between Scotland and Faroe still, however, remains the most conspicuous as well as the most accessible, and it is very important for us before concluding the Report of the *Challenger* Expedition, to have an opportunity of checking with our greatly increased knowledge our earlier observations.

"I have carefully considered what would be the minimum amount of work required for this purpose, and I now write to ask if you could, with the sanction of their Lordships, authorise Capt. Tizard, now surveying on the west coast, to run north to Stornoway and sound out the line indicated. This would occupy a month, or perhaps a little more.

"As remarkable differences in the distribution of marine animals accompany these differences in temperature, I should greatly regret if we had not a few casts of the trawl on each side of the line, but any additional expense involved for this purpose I will gladly meet. I regret greatly that my present state of health prevents my committing myself to accompany the vessel during the whole time, but I will be at Stornoway during the survey, and my chief assistant, Mr. Murray, is prepared to go. I should think that about the middle of July would be the best time for the trip, if that time would be convenient. Trusting for your assistance to the kind interest you have always taken in our work, believe me very truly yours,

"C. WYVILLE THOMSON"

I give this letter in full to show that our anticipations were very definite, although they were founded entirely upon the comparison of serial temperature soundings.

Their Lordships agreed to my proposal, and on July 22, 1880, I joined the *Knight Errant* at Oban, and proceeded to Gairloch, and thence to Stornoway, where we arrived at mid-day on Saturday, the 24th. The weather was delightful, and the Minch as smooth as glass; when we reached Stornoway, however, the barometer had begun to fall, and continued sinking steadily with a rising breeze from the north-east. After coaling on Monday forenoon, the vessel left Stornoway Harbour at 1 p.m. with a rather unfavourable weather forecast. I meant to have gone with her on this trip but I was advised to give up the idea, and the civilians who accompanied Capt. Tizard were Mr. Murray, our indefatigable assistant Mr. Frederick Pearcey, with my son as a supernumerary. Taking the island of North Rona as a point of departure, during Tuesday the 27th, and Wednesday the 28th, the *Knight Errant* ran a sectional line of soundings, the distance between the soundings averaging ten miles, between the shallow water on the Scottish coast and the bank to the south-west of the Faroe Islands. Fourteen soundings on this line gave the following depths and bottom temperatures:—

	Depth, fathoms.	Temp. °		Depth, fathoms.	Temp. °
1	88	49·5	8	405	46·0
2	178	49·6	9	355	43·8
3	400	45·8	10	270	43·5
4	560	45·2	11	335	41·0
5	540	46·0	12	245	41·8
6	300	47·5	13	120	47·5
7	305	46·5	14	130	46·0

The line was therefore entirely in the *warm area*, and no perceptible amount of water from the cold area could be shown to pass in this direction towards the Atlantic.

Capt. Tizard then proceeded a little way to the north-eastward, and commenced running a second line, parallel to the first and about eight miles from it, back towards the Scottish coast. Soundings were continued on the second line at the same average distances as before on Wednesday and Thursday morning, when, the barometer falling rapidly and the sea running high with a gale from the north-east, it was thought prudent to bear up for Stornoway, which they reached on Friday after a somewhat anxious twenty-four hours.

On Tuesday, August 3, the weather looking somewhat better, the *Knight Errant* left Stornoway and carried a sectional line north-north-west from Rona towards the last sounding; they completed the second line of soundings on the evening of Wednesday, the 4th inst. On this line twelve soundings gave depths and bottom temperatures according to the following table:—

	Depth, fathoms.	Temp. °		Depth, fathoms.	Temp. °
1	370	35·5	7	285	45·8
2	375	31·0	8	255	48·0
3	375	31·0	9	460	46·0
4	285	32·5	10	202	48·2
5	210	47·0	11	145	49·5
6	260	47·5	12	93	50·0

All these soundings therefore, with the exception of Nos. 1, 2, and 3, which were across the ridge in the *cold area*, and No. 9, which was in the deep water of the *warm area*, gave a depth of under 300 fathoms, and were consequently on the ridge.

On August 10 the *Knight Errant* went out again and got several fairly successful hauls of the trawl and a serial temperature-sounding in the *warm area*, returning on Thursday the 12th, and she left Stornoway for the fourth time on Monday, August 16, when the party landed on Rona and gave it a cursory examination. They then steamed towards the deep water of the *cold area*, and on Tuesday the 17th they trawled successfully, and took a serial temperature-sounding in 540 fathoms. They returned to Stornoway for the last time on the evening of Thursday, and left on the following day for Greenock, where they arrived on Monday, the 23rd.

The observations made by Capt. Tizard in the *Knight Errant* have fully corroborated the results of the *Lightning* and *Porcupine* as to the facts of the abnormal distribution of temperature in the Faroe Channel. They have also established the existence of a submarine ridge rising to within 300 fathoms of the surface, in the position in which such a ridge is required to satisfy the conditions of the doctrine of the interference of continuous barriers with the distribution of deep-sea temperatures. Thus far they may be regarded as entirely successful.

The highest line of the ridge has probably not been found, and the details of temperature have yet to be traced out more accurately along the line and for a short distance on either side. I consider that it would be of the greatest interest to work this case out fully as a striking example, within a few miles of our own shore, of a physical phenomenon of importance from its wide occurrence.

The *Knight Errant* was found quite unsuitable for such work; a small steamer of ordinary strength, with stowage for coals for a fortnight's steaming, and with sails to enable her to lie to in a breeze, could do all that is required within a month or six weeks of the ordinary variable weather of these seas.

Although the solution of this temperature problem was the principal object of this summer's trip, I wished greatly to make some additional observations on the nature of the faunæ on the two sides of the ridge, and I was especially anxious to procure some fresh specimens of sponges as material for the structural part of the memoir in which I am engaged with Prof. Franz Eilhart Schultze on the Hexactinellidæ of the *Challenger* Expedition.

The Admiralty declined to give us any material assistance in this direction, but they allowed me to take the gear on board and to get a cast of the trawl or dredge in the intervals of sounding, or when the sounding work was over. I accordingly provided 1,000 fathoms of 2½ inch dredge-rope and other necessary appliances, and Messrs. Henderson, ship-builders, Glasgow, kindly lent us an excellent steam-winch, which was fitted on deck and was of the greatest service both in sounding and trawling. Owing to the boisterous weather and insufficiency of the vessel, this part of our undertaking was not very successful. I got none of the coveted sponges, but two or three hauls of the trawl were taken in each area, and a number of highly-characteristic abyssal forms were procured, including some deep-sea fishes, several crustaceans, and a number of gigantic pycnogonids, some interesting echinoderms, including *Porocidaris*, *Asthenosoma*, *Phormosoma*, *Pourtalesia*, *Rhizocrinus*, and others; some corals, and many curious rhizopods. As the vessel has not yet returned, I take these names from Mr. Murray's rough notes.

Enough has been done to give further evidence, if such were needed, that a small district on the northern slope of the coast of Scotland will afford a richer harvest to a properly-organised dredging-excursion than perhaps any other spot on the earth. The whole area is singularly productive, and it is bisected by a narrow line, on the one side of which the warm sea at a depth of 500 to 600 fathoms vies in abundance and variety of abyssal forms with the favoured patches off Inosima and Zebu; while on the other side of the line, within a distance of a few miles, we find an epitome of the fauna of the depths of the Arctic Sea.

C. WYVILLE THOMSON

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

A Fragment of Primeval Europe

THE paper in NATURE, vol. xxii. p. 400, by Prof. Archibald Geikie, on the glacial phenomena of the north-west coast of Scotland, contains on many points a most true and graphic description of a most peculiar and a most interesting country. But I demur to its accuracy on one of the main features to which he refers. The amount of glaciation on the hills of Laurentian gneiss, as represented in the sketch on p. 401, is inordinately exaggerated. I know that country well, both in its general aspect and in its details, and no part of it presents such a scene of symmetrically rounded hills, like the huts of Caffres in Zululand, as that depicted in the sketch.

It is true that all the lower hills are more or less strongly glaciated. But they are also full of low cliffs, and precipitous rocks upon the sides of the glens, and the whole character of the glaciation is such as to suggest the action of heavy floating ice such as that of the "Palæocrystic Sea," and which acted only upon surfaces specially exposed.

Ben Stack, which is 2,364 feet high, and is composed of the same rock, is not rounded at all, and on the north-west face is full of great precipices along which no glaciation can be seen.

It is perfectly true that the same glaciation which is common on the exposed surfaces of the gneiss cannot be traced on the Cambrian sandstones which overlie it. But this is probably due to the obliteration of the ice-marks by subsequent atmospheric action, which tells rapidly and powerfully on the sandstones, whilst it is almost inoperative on the intensely hard and tough Laurentian gneiss.

That this is the true explanation of the difference now presented by the two rocks, is evident from the fact that the next rock in the ascending series, namely, the white quartzites, do retain surfaces in abundance which are splendidly glaciated. I know no spot in Scotland where the polished surfaces due to glaciation are seen on a greater scale than on the top of the white quartzites which cap the mountain of Queenaig in Assynt. This is a classical area in geology—a sketch of it forming the frontispiece of Murchison's "Siluria." The road from Inchnadamf to Kylescue and Scourie passes over a plateau formed of this quartzite, and the beds of white rock, highly glaciated, shine for miles through the heather.

The glaciation which left these surfaces must have passed over the sandstones also. But the rock was not of a material calculated to retain the marks.

Nevertheless I am not prepared to deny that possibly the gneiss of Sutherland may have been doubly glaciated—once in the glacial epoch as hitherto known to geology, and also at some former epoch inconceivably remote, when similar conditions had prevailed.

If well glaciated surfaces of the gneiss can be distinctly traced

passing under the Cambrian conglomerates, such evidence would go a great way. I have examined some spots where the Cambrian conglomerate has left cakes sitting on the gneiss, and at these spots I should say decidedly that there was no proof of the glaciation of the subjacent rock.

That there may be such evidence at other points is very possible; and if Mr. Geikie can establish it he will have made a discovery of high interest in geology. ARGYLL

August 27

New Red Star

MR. ORMOND STONE, writing from Mount Lookout, U.S., lately informed me that on August 6 he found a very red star, 6·5 mag. in 19h. 10m. ±, and − 16° 7′ ±. I observed it last night, when it appeared, according to my estimation, of no more than 7·5–8 magnitude. In colour it ranks among the most remarkable red stars, and as it is also, very probably, a variable, I would recommend it to the close attention of observers. It agrees approximately in R.A. with the well-known variable T Sagittarii, but differs in more than a degree of declination from that star, of which I find the place in my *Red Star Catalogue* to be α, 19h. 9m. 19s.; and δ, −17° 10′·7 for 1880. In about that position last night I found a small colourless star not more than 10 or 11 magnitude. JOHN BIRMINGHAM

Millbrook, Tuam, August 29

Locusts and Coffee Trees

MR. S. B. O'LEARY of this city has favoured me with extracts from a letter written by a relation of his residing on a plantation near Antigua-Guatemala, and containing information about the locust-plague, by which lately the crops of Indian corn and a great many coffee plantations in that country have been destroyed. The insect is called *Chapulin* (*Gryllus miles*, Drury?), and appeared first in the department of Chiquimula, in the eastern part of Guatemala, close to Honduras. Thence it spread over all the warmer parts of the Republic, avoiding the higher and cooler regions. The loss must be very considerable; one gentleman, Don Gregorio Revuelto, in the department of Suchitepeque, lost in one night 70,000 trees, without there being left one single leaf. In April a swarm, supposed to be four leagues broad and about 300 metres long, approached the estate belonging to the writer of the letter, but fortunately could be partly driven away with noise and smoke.

These facts are interesting, as it has not been observed hitherto that locusts, in such a degree, attack the coffee-tree.

Carácas, August 2 A. ERNST

Intellect in Brutes

A VERY interesting instance of animal intelligence has been recently reported in one of our newspapers, and may appear sufficiently remarkable to merit more extended notice. A large and destructive fire lately took place upon the shores of the East River opposite to New York, between which city and Long Island this channel passes. The occasion was the spread of naphtha from a burning oil-ship, which instantly became a trail of fire from which the flames swept into well-filled lumber-yards covered with pine boards, and thence to the loaded barges which lined the docks along the river front, and extended up the banks of a small neighbouring creek.

By the rapid and uncontrolled advance of the conflagration over this wide area the families and occupants of the barges and in the lumber-yards were driven away and forced to seek safety in flight. A Newfoundland dog belonging to the grounds, and at that time roaming amongst the lanes of lumber, found himself imprisoned by a swiftly-contracting arc of flame, with the river on one side as the single avenue of escape. Unlike the beasts in the notable dilemma of Baron Munchausen, these opposed elements refused to leap over the back of their prey, and, extinguishing each other, permit him to escape.

The dog jumped into the water and headed for the opposite (the New York) shore. Although pursued by men in boats and lured by cries and calls from the shores, he steadily kept on his course, and after a long and difficult trip landed on the New York side of the water. From the shore he reached one of the avenues which run lengthwise through New York, and finally found his way to the 34th Street ferry, which lay at a considerable distance below the point of his landing. The dog, following the lumber-waggons, had often crossed from one shore to the other by means of this ferry, and now recognised in his present extremity, he easily secured a single passage.

Once returned to the Long Island side, he regained his old quarters, having by this circuitous route baffled the fire and regained his home. L. P. GRATACAP

Amer. Mus. Nat. Hist., N.Y., August 14

CHATEL, Jersey, must send a more precise address.

THUNDERSTORMS[1]

III.

THE name *thunderbolt*, which is still in use, even by good writers, seems to have been introduced in consequence of the singular effects produced when lightning strikes a sandhill or sandy soil. It bores a hole often many feet in length, which is found lined throughout with vitrified sand. The old notion was that an intensely hot, solid mass, whose path was the flash of lightning, had buried itself out of sight, melting the sand as it went down. It is quite possible that this notion may have been strengthened by the occasional observation of the fall of aerolites, which are sometimes found, in the holes they have made, still exceedingly hot. And at least many of the cases in which lightning is said to have been seen in a perfectly clear sky are to be explained in the same way. Every one knows Horace's lines—

> "Diespiter
> Igni corusco nubila dividens
> Plerumque, per purum tonantes
> Egit equos volucremque currum."

But Virgil's remark is not so commonly known. He is speaking of prodigies of various kinds, and goes on:—

> "Non alias coelo ceciderunt plura sereno
> Fulgura; nec diri toties arsere cometae."

It is very singular that he should thus have associated comets and meteorites, which quite recent astronomical discovery has shown to have a common origin.

Another remarkable peculiarity, long ago observed, is the characteristic smell produced when lightning strikes a building or a ship. In old times it was supposed to be sulphurous; nowadays we know it to be mainly due to ozone. In fact, all the ready modes of forming ozone which are as yet at the disposal of the chemist depend upon applications of electricity. But besides ozone, which is formed from the oxygen of the air, there are often produced nitric acid, ammonia, and other compounds derived from the constituents of air and of aqueous vapour. All these results can be produced on a small scale in the laboratory.

Hitherto I have been speaking of lightning discharges similar in kind to the ordinary electric spark, what is commonly called *forked* or *zig-zag* lightning. Our nomenclature is very defective in this matter, and the same may be said of the chief modern European languages. For, as Arago remarks, by far the most common form of lightning flash observed in thunderstorms is what we have to particularise, for want of a better term, as *sheet-lightning*. He asserts that it occurs thousand-fold as often as forked lightning; and that many people have never observed the latter form at all! It is not at all easy to conceive what can be the nature of sheet-lightning, if it be not merely the lighting up of the clouds by a flash of forked lightning not directly visible to the spectator. That this is, at least in many cases, its origin is evident from the fact that its place of maximum brightness often takes the form of the *edge* of a cloud, and that the *same* cloud-edge is occasionally lit up several times in quick succes-

[1] Abstract of a lecture, delivered in the City Hall, Glasgow, by Prof. Tait. Continued from p. 365.

sion. You will remember that we are at present dealing with the appearances observed *in* a thunderstorm, so that I do not refer to that form of sheet-lightning which commonly goes by the name of *summer-lightning*, and which is not, audibly at least, followed by thunder.

The next remarkable feature of the storm is the thunder, corresponding, of course, on the large scale, to the snap of an electric spark. Here we are on comparatively sure ground, for sound is very much more thoroughly understood than is electricity. We speak habitually and without exaggeration of the *crash* of thunder, the *rolling* of thunder, and of a *peal* of thunder; and various other terms will suggest themselves to you as being aptly employed in different cases. All of these are easily explained by known properties of sound. The origin of the sound is in all cases to be looked for in the instantaneous and violent dilatation of the air along the track of the lightning flash, partly, no doubt, due to the disruptive effects of electricity of which I have already spoken, but mainly due to the excessive rise of temperature which renders the air for a moment so brilliantly incandescent. There is thus an extremely sudden compression of the air all round the track of the spark, and a less sudden, but still rapid, rush of the air into the partial vacuum which it produces. Thus the sound-wave produced must at first be of the nature of a bore or breaker. But as such a state of motion is unstable, after proceeding a moderate distance the sound becomes analogous to other loud but less violent sounds, such as those of the discharge of guns. Were there few clouds, were the air of nearly uniform density, and the flash a short one, this would completely describe the phenomenon, and we should have a thunder *crash* or thunder *clap* according to the greater or less proximity of the seat of discharge. But, as has long been well known, not merely clouds but surfaces of separation of masses of air of different density, such as constantly occur in thunderstorms, *reflect* vibrations in the air; and thus we may have many successive echoes, prolonging the original sound. But there is another cause, often more efficient than these. When the flash is a long one, all its parts being nearly equidistant from the observer, he hears the sound from all these parts simultaneously; but if its parts be at very different distances from him, he hears *successively* the sounds from portions farther and farther distant from him. If the flash be much zig-zagged, long portions of its course may run at one and the same distance from him, and the sounds from these arrive simultaneously at his ear. Thus we have no difficulty in accounting for the *rolling* and *pealing* of thunder. It is, in fact, a mere consequence, sometimes of the reflection of sound, sometimes of the finite velocity with which it is propagated. The usual rough estimate of five seconds to a mile is near enough to the truth for all ordinary calculations of the distance of a flash from the observer.

The extreme distance at which thunder is heard is not great, when we consider the frequent great intensity of the sound. No trustworthy observation gives in general more than about nine or ten miles, though there are cases in which it is possible that it may have been heard fourteen miles off. But the discharge of a single cannon is often heard at fifty miles, and the noise of a siege or naval engagement has certainly been heard at a distance of much more than 100 miles. There are two reasons for this: the first depends upon the extreme suddenness of the production of thunder; the second, and perhaps the more effective, on the excessive variations of density in the atmosphere, which are invariably associated with a thunderstorm. In certain cases thunder has been propagated, for moderate distances from its apparent source, with a velocity far exceeding that of ordinary sounds. This used to be attributed to the extreme suddenness of its production; but it is not easy, if we adopt this hypothesis, to see why it should not occur in all cases. Sir W. Thomson has supplied a very different explanation, which requires no unusual velocity of sound, because it asserts the production of the sound *simultaneously* at all parts of the air between the ground and the cloud from which the lightning is discharged.

We now come to an exceedingly strange and somewhat rare phenomenon, to which the name of *fire-ball* or *globe-lightning* has been given. As we are as yet unable to produce anything of this kind by means of our electrical machines, some philosophers have tried to cut the Gordian knot of the difficulty by denying that any such thing can exist. But, as Arago says, "*Où en serions nous, si nous nous mettions à nier tout ce qu'on ne sait pas expliquer?*" The amount of trustworthy and independent evidence which we possess as to the occurrence of this phenomenon is such as must convince every reasonable man who chooses to pay due attention to the subject. No doubt there is a great deal of exaggeration, as well as much imperfect and even erroneous observation, in almost all of these records. But the existence of the main feature (the fire-*ball*) seems to be proved beyond all doubt.

The most marked peculiarities of this species of lightning-discharge are its comparatively long duration and its comparatively slow motion. While a spark, or lightning-flash, does not last longer than about a millionth part of a second, if so long, globe-lightning lasts from one to ten seconds, sometimes even longer, so that a sufficiently self-possessed spectator has time carefully to watch its behaviour. The general appearance is that of a luminous ball, which must be approximately spherical, because it always appears circular in outline, slowly and steadily descending from a thundercloud to the ground. It bursts with a loud explosion, sometimes before reaching the ground, sometimes as it impinges, and sometimes after actually rebounding. Its size varies from that of a child's head to a sphere of little less than a yard in diameter. On some occasions veritable flashes of lightning were seen to proceed from large fire-balls as they burst. It is difficult to imagine what these balls can be if they be not a species of natural Leyden jar very highly charged. If it be so, no ordinary lightning-rod can possibly prevent danger from it; and we may thus be able to explain the very few cases in which damage has been done by lightning to thoroughly protected buildings. To guard against this form nothing short of a pretty close net-work of stout copper wires would suffice. Meanwhile I give a brief sketch of *two* out of the long series of descriptions of such phenomena which Arago has patiently collected. The first is given on the authority of Babinet, who was deputed by the Academy of Sciences to make inquiries into the case.

Shortly, but not immediately, after a loud peal of thunder, a tailor who was sitting at his dinner saw the paper ornament which covered his fire-place blown down as if by a gentle breeze, and a globe of fire, about the size of a child's head, came gently out and moved slowly about at a slight elevation above the floor. It appeared bright rather than hot, and he felt no sensation of warmth. It approached him like a little kitten which desired to rub itself in play against his legs; but he drew his feet away, and by slow and cautious movements avoided contact with it. It remained several seconds near his feet, while he leaned forward and carefully examined it. At last it rose vertically to about the level of his head, so he threw himself back in his chair and continued to watch it. It then became slightly elongated, and moved obliquely towards a hole pierced to the chimney about a yard above the mantelpiece. This hole had been made for the chimney of a stove which was used in winter. "But," as the tailor said, "the globe could not *see* the hole, for paper had been pasted over it." The globe went straight for the hole, tore off the paper, and went up the chimney. After the lapse of time which, at the rate at which he had seen it moving, it would have required to get to the

top of the chimney, a terrific explosion was heard, and a great deal of damage was done to the chimney and the roofs around it.

The next is even more striking: In June, 1849, in the evening of one of the days when cholera was raging most formidably in Paris, the heat was suffocating, the sky appeared calm, but summer lightning was visible on all sides. Madame Espert saw from her window something like a large red globe, exactly resembling the moon when it is seen through mist. It was descending slowly towards a tree. She at first thought it was a balloon, but its colour undeceived her; and while she was trying to make out what it was she saw the lower part of it take fire ("*Je vis le feu prendre au bas de ce globe*"), while it was still some yards above the tree. The flames were like those of paper burning slowly, with sparks and jets of fire. When the opening became twice or thrice the size of one's hand, a sudden and terrific explosion took place. The infernal machine was torn to pieces, and a dozen flashes of zig-zag lightning escaped from it in all directions. The *débris* of the globe burned with a brilliant white light and revolved like a catherine-wheel. The whole affair lasted for at least a minute. A hole was bored in the wall of a house, three men were knocked down in the street, and a governess was wounded in a neighbouring school, besides a good deal of other damage.

I have never seen one myself, but I have received accounts of more than one of them from competent and thoroughly credible eye-witnesses. In particular on a stormy afternoon in November, 1868, when the sky was densely clouded over, and the air in a highly electrical state, there was heard in Edinburgh one solitary short, but very loud, clap of thunder. There can be no doubt whatever that this was due to the explosion of a fire-ball, which was seen by many spectators, in different parts of the town, to descend towards the Calton Hill, and to burst whilst still about a hundred feet or so above the ground. The various accounts tallied in most particulars, and especially in the very close agreement of the positions assigned to the ball by spectators viewing it from different sides, and in the intervals which were observed to elapse between the explosion and the arrival of the sound.

The remaining phenomena of a thunderstorm are chiefly the copious fall of rain and of hail, and the almost invariable lowering of the barometer. These are closely connected with one another, as we shall presently see.

(*To be continued.*)

THE BRITISH ASSOCIATION

WE had but just time before going to press last week to indicate the general arrangements made for the reception at Swansea of the British Association. We have now to chronicle the events of the meeting which, although small, has not been destitute of many points of interest. The actual number of members and associates in attendance has been smaller than is shown by the returns for many previous years. This is probably accounted for by the geographical isolation of Swansea and the smallness of its population; but there are doubtless other collateral causes—such, for example, as the coincidence of the meeting of the Iron and Steel Institute at Düsseldorf—which have contributed to discourage a large attendance.

At the General Committee meeting on August 25 the Report of the Council for the year 1879-80 was presented.

The Council having been requested by the General Committee at Sheffield to take such further action as regards the correspondence with the Treasury about the Natural History Collections as they should think desirable in the interests of science, prepared and sent to the Secretary of the Treasury, in reply to his letter of July 22, 1879, the following letter:—

"*British Association for the Advancement of Science,*
"*22, Albemarle Street, London, W., June* 8, 1880

"SIR,—The letter of the Council of this Association, of March 25, 1879, respecting the administration of the Natural History Collections, and your reply thereto of July 22, have been laid before the British Association, at the meeting held at Sheffield in August last, when the subject was again referred to the Council.

"On the part of the Council I am now requested to inform you that they learn with satisfaction that the action of Her Majesty's Government, in passing the British Museum Act of 1878, does not prejudice the question of the future administration of the Natural History Collections at South Kensington, but that the subject is still under the consideration of the Lords Commissioners of Her Majesty's Treasury.

"Under these circumstances the Council of the Association must again express their hope that, when the period arrives, as it must shortly do, for the settlement of the question, the recommendations of the Royal Commission on Science will have their full weight and importance accorded to them.

"If, however, the Lords Commissioners of Her Majesty's Treasury are prepared, as they would seem to indicate, to constitute a Special Standing Committee, or Sub-Committee, of the Trustees of the British Museum, for the management of the Natural History Collections, the Council of the Association are of opinion that such a form of government, though not the form suggested by the Royal Commission on Science, might possibly be so organised as to be satisfactory both to the public and to men of science.

"Trusting that the Lords Commissioners will do the Council the favour of considering these observations on a subject which keenly interests many members of the British Association,

"I have the honour to be, sir,
"Your obedient servant,
"G. J. ALLMAN,
"*President of the British Association for the Advancement of Science.*
"SIR R. R. W. LINGEN, K.C.B., &c., &c."

The receipt of this letter has been acknowledged.

The Council have elected Prof. Cornu, of Paris, and Prof. Boltzmann, of Vienna, Corresponding Members since the Sheffield meeting.

The president's address was very well received; and suffered nothing from the extempore style adopted by Prof. Ramsay, who held his audience to the end. The presidential addresses of the sections were of an unusually high order, and happily no hitch occurred this year to necessitate the delay of any of them beyond the day and hour fixed for their delivery. We reprinted some of them last week, and others will be found in our columns to-day. Of papers in the different sections there has not been, perhaps, as plentiful a supply as in other years, Section G exhibiting a decided lack. There has been certainly a less amount of illustration by diagrams and experiments in the Section meetings than is usual, though several papers in Section A were accompanied by the exhibition of new forms of apparatus. The Neanderthal skull exhibited by Prof. Schaffhausen of Bonn in Section D drew an inquisitive crowd of admirers. The excursions to the local centres of industry—the Dowlais Works, the Landore Steel Works, and the various copper and tin works thrown open by the courtesy of their proprietors—were deservedly popular. Some of these specially invited the notice of Section G, which on the very first day of meeting adjourned for a visit to Dowlais. The promoters of the new East Dock have not omitted to seek for possible shareholders in the members of the Association.

The Saturday excursions to Milford, Gower, St.

David's, &c., were attended by a fair number of visitors, but the exertions of the Excursion Committees had made more than ample accommodation for the limited number of excursionists. The excursions announced for to-day were numerous, and of an interesting character; but several of them have been withdrawn, in consequence of the smallness of the number of applications.

The first of the two evening *soirées* was a reception by the Mayor of Swansea, and was enlivened by some excellent music by a local chorus and orchestra. There were a number of exhibits of machinery in motion, and of products of local industries.

The second of the *soirées* was held on the evening of Tuesday, the 31st, and was more particularly devoted to scientific apparatus. The temporary pavilion in Burrows Square, in which these entertainments were given, was illuminated by the light of the Jamin candle lately described in NATURE, giving an agreeable and brilliant light, though somewhat unsteady, under the disadvantageous conditions under which it had to be set up and worked.

The evening lectures cannot be said to have been largely patronised, although perhaps the geographical difficulties of the town may have accounted to some extent for this state of things. Prof. Boyd Dawkins's discourse on "Primæval Man," excellently delivered and admirably illustrated, was very attentively listened to and enthusiastically applauded. Mr. F. Galton's lecture on "Mental Imagery" was illustrated by diagrams of some of the singular generic photographs with which he has identified his name; great amusement being evoked by the exhibition of the face of a "generalised" Welsh Dissenting minister, compounded from photographs of sundry Nonconformist divines of Swansea. Mr. F. Seebohm lectured on the North-east Passage to a small but very attentive audience of working men on Saturday evening, dwelling more particularly on the heroic exploits of Nordenskjöld and of Capt. Willing.

At the meeting of the General Committee on Monday arrangements were made for the holding next year at York of the jubilee gathering of the Association. The proceedings were unusually interesting and enthusiastic.

Sir John Lubbock was chosen as President, the Vice-Presidents being His Grace the Archbishop of York, and those Past-Presidents of the Association who were living when it was founded in 1831. As Presidents of the Sections the following have been chosen, all of whom are Past-Presidents of the Association:—Mathematics and Physics, Sir William Thomson; Chemistry, Prof. Williamson; Geology, Prof. Ramsay; Biology, Prof. Owen; Geography, Sir J. Hooker; Mechanics, Sir Wm. Armstrong.

After a stout competition between Southampton, Nottingham, and Southport, the first-mentioned of these places was selected for the meeting of 1882.

At the final meeting of the Association on Wednesday, the following grants were made:—

A—Mathematics and Physics	£
Prof. G. C. Foster—Electrical Standards	100
Mr. G. H. Darwin—Lunar Disturbance of Gravity	30
Prof. Everett—Underground Temperatures	20
Dr. Joule—Mechanical Equivalent of Heat	40
Dr. O. J. Lodge—High Insulation Key	5
Sir Wm. Thomson—Seismic Experiments	30
Sir Wm. Thomson.—Tidal Observation	10
Mr. J. M. Thomson.—Inductive Capacity of Crystals and Paraffin	10
B—Chemical Science	
Dr. Gladstone.—Specific Refractions	10
Lord Rayleigh.—Spectrum Analysis	10
C—Geology.	
Prof. Duncan—Fossil Polyzoa	10
Mr. J. Evans—Geological Record	100
Prof. E. Hull—Underground Waters	£10
Prof. A. C. Ramsay—Earthquake in Japan	25
Dr. Sorby—Metamorphic Rocks	10
D—Biology	
Dr. M. Foster—Scottish Zoological Station	50
Dr. M. Foster—Naples Zoological Station	75
Mr. Godwin Austen—Natural History of Socotra	50
Prof. Gwyn Jeffreys—Exploration of Sea Bed North of Hebrides	50
General Pitt Rivers—Anthropological Notes	20
Dr. Pye Smith.—Elimination of Nitrogen during Bodily Exercise	50
Mr. P. L. Sclater.—Natural History of Timor Laut	50
Mr. Stainton.—Zoological Record	100
F—Statistics and Economic Science	
Mr. F. Galton.—Estimation of Weights and Heights of Human Beings	30
G—Mechanical Science	
Mr. Bramwell.—Patent Laws	5
Mr. J. Glaisher.—Wind Pressure	5
Prof. O. Reynolds.—Steering Steamships	5

SECTION A

MATHEMATICS AND PHYSICS

OPENING ADDRESS BY PROF. W. GRYLLS ADAMS, M.A., F.R.S., PRESIDENT OF THE SECTION

IT has been said by a former President of this Section of the British Association that the president of a Section ought to occupy your time, not by speaking of himself or his own feelings, but by a review "more or less extensive of those branches of science which form the proper business of his section." He may give a rapid sketch of the progress of mathematical science during the year, or he may select some one special subject, or he may take a middle course, neither so extensive as the first nor so limited as the second.

There are many branches of science which have always been regarded as properly belonging to our section, and the range is already wide; but it is becoming more and more true every day that the sciences which are dealt with in other sections of the Association are becoming branches of Physics, *i.e.*, are yielding results of vast importance when the methods and established principles of Physics are applied to them. I wish to direct your attention to investigations which are being made in that fertile region for discovery, the "border land" between Chemistry and Physics, where we have to deal with the constitution of bodies, and where we are tempted to speculate on the existence of matter and on the nature of the forces by which the different parts of it are bound together, or become so transformed that all resemblance to their former state is lost. It is not long since the theory of exchanges became thoroughly recognised in the domain of Radiant Heat, and yet it is already recognised and accepted in the theory of Chemical Combination. Just as the molecules of a body which remains at a constant temperature are continuously giving up their heat motion to surrounding molecules, and getting back from them as much motion of the same kind in return, so in a chemical compound which does not appear to be undergoing change, the combining molecules are continuously giving up their chemical or combining motions to surrounding molecules, and receiving again from them as much combining motion in return. We may say that each molecule is, as far as we can see, constantly dancing in perfect time with a partner, and yet is continuously changing partners. When such an idea of chemical motion is accepted, we can the more easily understand that chemical combination means the alteration of chemical motion which arises from the introduction of a new element into the space already occupied, and the consequent change in the motion of the new compound as revealed to us in the spectroscope. We can also the more readily understand that in changing from the old to the new form or rate of motion there may be a development of energy in the shape of heat-motion which may escape or become dissipated wherever a means of escape presents itself. We know from the experiments of M. Favre that as much heat is absorbed during the decomposition

of an electrolyte as is given out again by the combination of the substances composing it.

We are making rapid strides towards the exact determination of those relations between the various modes of motion or forms of energy which were so ably shadowed forth, and their existence established long ago, by Sir William Grove in his "Correlation of the Physical Forces," where, in stating the conclusion of his comparison of the mutual interchange of physical forces, he distinctly lays down the principles of energy in this statement: "Each force is definitely and equivalently convertible into any other; and where experiment does not give the full equivalent, it is because the initial force has been dissipated, not lost, by conversion into other unrecognised forces. The equivalent is the limit never practically reached."

The laws of Faraday, that (1) when a compound is electrolysed the mass of the substance decomposed is proportional to the quantity of electricity which has produced the change, and that (2) the same current decomposes equivalent quantities of different substances, *i.e.*, quantities of their elements in the ratio of their combining numbers, have given rise to several determinations of the relation between chemical affinity and electromotive force. In a paper lately communicated to the Physical Society, Dr. Wright has discussed these several determinations, and has given an account of a new determination by himself. The data at present extant show that when one gramme of hydrogen unites with 7·98 grammes of oxygen, there are about 34,100 units of heat given out, making the latent heat of dissociation of one gramme of water equal to 3,797 units. The results obtained are compared with the heat given out by the combustion of hydrogen and oxygen, and the value of the mechanical equivalent of heat is deduced from these determinations.

The value obtained by Dr. Wright, which depends on the value of Clark's standard cell, and therefore depends upon the value of the ohm, agrees fairly well with Joule's determination from the heat produced by an electric current in a wire, but is greater than Joule's value as obtained from his water-friction experiments. This may be accounted for by supposing an error in the value of the ohm or B.A. unit, making it too large by 1·5 or 2 per cent. Kohlrausch has also made comparisons of copies of the B.A. unit with standard coils, and comes to the conclusion that the B.A. unit is 1·96 per cent. too large. On the other hand, Prof. Rowland, in America, has made a new determination, and finds that according to his calculations the B.A. unit is nearly 1 per cent. too small. These differences in the values obtained by different methods clearly point to the necessity for one or more new determinations of the unit, and I would venture to suggest that a determination should be made under the authority of this Association, by a committee appointed to carry out the work. And it is not sufficient that this determination should be made once for all, for there is reason to think that the resistance of standard coils alters with time, even when the material has been carefully selected. It has been found that coils of platinum silver which were correct copies of the standard ohm have become so altered, and have their temperature coefficients so changed, that there are doubts as to the constancy of the standards themselves. Pieces of platinum-silver alloy cut from the same rod have been found to have different temperature coefficients. The value ·031 for 1° C. is given by Matthiessen for this alloy, yet two pieces of wire drawn from the same rod have given, one ·021 per cent. and the other ·04 per cent. for 1° C. Possibly this irregularity in the platinum-silver alloys may be due to something analogous to the segregation which Mr. Roberts has found to take place in copper-silver alloys in their molten state, and which Matthiessen in 1860 regarded as mechanical mixtures of allotropic modifications of the alloy.

A recommendation has been made that apparatus for determining the ohm should be set up in London, and that periodically determinations be made to test the electrical constancy of the metals and alloys used in making coils. A committee should be authorised to test coils and issue certificates of their accuracy, just as is done by the Kew Committee with regard to meteorological instruments. The direct relation between Heat and Chemical work has been established, and the principles of Conservation of Energy been shown to be true in Chemistry by the experiments of Berthelot and of Thomsen, so that we may say that when a system of bodies passes through any succession of chemical changes, the heat evolved or absorbed when no external mechanical effect is produced depends solely upon the initial and final tates of the system of bodies, whatever be the nature or the order of the transformations. The extension of this principle to the interaction of the molecules and atoms of bodies on one another is of vast importance in relation to our knowledge of the constitution of matter, for it enables us to state that each chemical compound has a distinct level or potential which may be called its own, and that when a compound gives up one of its elements to another body, the heat evolved in the reaction is the difference between the heat of formation of the first compound and that of the resulting product.

We have become accustomed to regard matter as made up of molecules, and those molecules to be made up of atoms separated from one another by distances which are great in comparison with the size of the atom, which we may regard as the smallest piece of matter that we can have any conception of. Each atom may be supposed to be surrounded by an envelope of ether which accompanies it in all its movements. The density of the ether increases rapidly as an atom is approached, and it would seem that there must be some force of attraction between the atom and its ether envelope. All the atoms have motions of translations in all possible directions, and according to the theories of Maxwell and Boltzmann, and the experiments of Kundt, Warburg, and others on the specific heat of vapours, in *one-atom* molecules in the gaseous state there is no motion of rotation. According to the theory of Pictet, the liquid state, being the first condensation from the gaseous state, must consist of at least two gaseous atoms combined. These two atoms are bound to one another through their ether envelopes. Then the solid state results from the condensation of a liquid, and so a solid molecule must consist of at least two liquid molecules, *i.e.* at least four gaseous molecules, each surrounded by an atmosphere of ether. M. Pictet imagines these atoms to be centres of attraction; hence in the solid with four such centres the least displacement brings into action couples tending to prevent the molecule from twisting as soon as external forces act upon it. All the molecules constituting a solid will be rigidly set with regard to one another, for the least displacement sets in action a couple or an opposing force in the molecules on one another.

Let us now follow the sketch which M. Pictet has given of changes which we may consider it to undergo when we expend energy upon it. Suppose a solid body is at absolute zero of temperature, which may be regarded as the state in which the molecules of a body are in stable equilibrium and at rest, the application of heat gives a vibratory motion to the molecules of the solid, which increases with the temperature, the mean amplitude of vibration being a measure of the temperature. We may regard the sum of all the molecular forces as the specific heat of the body, and the product of the sum of all the molecular forces by the mean amplitude of the oscillations; *i.e.* the product of the specific heat and the temperature will be the quantity of heat or the energy of motion of the body. As more and more heat is applied, the amplitude of vibration of the molecules increases until it is too great for the molecular forces, or forces of cohesion, and the melting point of the solid is reached. Besides their vibratory motion, the molecules are now capable of motions of translation from place to place among one another. To reduce the solid to the liquid state, *i.e.* to make the amplitude of vibration of the molecules sufficient to prevent them from coming within the sphere of the forces of cohesion, requires a quantity of heat which does not appear as temperature or molecular motion, and hence it is termed the latent heat of fusion. The temperature remains constant until the melting is complete, the heat being spent in bursting the bonds of the solid. Then a further application of heat increases the amplitude of vibration, or raises the temperature of the liquid at a rate depending on its specific heat until the succession of blows of the molecules overcomes the external pressure and the boiling-point is reached An additional quantity of heat is applied which is spent in changing the body to a gas, *i.e.*, to a state of higher potential, in which the motion of translation of the molecules is enormously increased. When this state is attained, the temperature of the gas again begins to increase, as heat is applied, until we arrive at a certain point, when dissociation begins, and the molecules of the separate substances of which the body is composed have so large an amplitude of vibration that the bond which unites them can no longer bring them again into their former positions. The potential of the substances is again raised by a quantity which is proportional to its chemical affinity. Again, we may increase the amplitude of vibration, *i.e.*, the temperature of the molecules, and imagine the possibility of higher and higher degrees of dissociation.

If temperature means the amplitude of vibration of the molecules, then we might expect that only those bodies which have their temperatures increased by the same amount when equal amounts of heat are applied to them can possibly combine with one another; and so the fact that the increase of temperature bears a fixed ratio to the increase of heat may be the cause in virtue of which bodies can combine with one one another. Were other bodies to begin to combine together at any definite temperature, they would immediately be torn to pieces again when the temperature is even slightly raised, because the amplitudes of vibration of their molecules no longer remain the same. This idea of temperature is supported by the fact that a combining molecule of each substance requires the same amount of heat to raise its temperature by the same number of degrees, the atomic weights being proportional to the masses of the combining molecules. The celebrated discovery of Faraday, that in a voltameter the work done by an electric current always decomposes equivalent quantities of different substances, combined with the fact that in the whole range of the physical forces work done is equivalent to the application of heat, is quite in accordance with the view that no molecule can combine with another which has not its amplitude of vibration altered by the same amount when equal quantities of heat are applied to both. As soon as we get any divergence from this state of equal motions for equal increments of heat, then we should expect that a further dissociation of molecules would take place, and that only those which are capable of moving together can remain still associated.

Just as in the change of state of a body from the solid to the liquid, or from the liquid to the gas, a great amount of heat is spent in increasing the motion of translation of the molecules without altering the temperature, so a great amount of heat is spent in producing dissociation without increasing the temperature of the dissociated substances, since the principle of conservation of energy has been shown by M. Berthelot to hold for the dissociation of bodies. We may conveniently make use of the term latent heat of dissociation for the heat required to dissociate a unit of mass of a substance.

We may thus sum up the laws of physical and chemical changes:—

1. All the physical phenomena of change of state consist in the subdivision of the body into molecules or particles identical with one another.

2. The reconstitution of a body into a liquid or a solid being independent of the relative position of the molecules, only depends on the pressure and temperature.

3. Dissociation separates bodies into their elements, which are of different kinds, and the temperature remains constant during dissociation.

4. The reunion of dissociated bodies depends on the relative position of the elements, and so depends on the grouping of the molecules. The atomic weight being the mass of a molecule as compared with hydrogen, the specific volume, *i.e.*, the atomic weight divided by the density, is the volume or *mean free path* of a molecule.

Building up his theory of heat on these principles, M. Pictet arrives at a definite relation between the atomic weight of a body, its density, its melting-point, and its coefficient of expansion, which may be stated thus—

The volume of a solid body will be increased as the temperature rises by an amount which is proportional to the number of molecules in it, and inversely as its specific heat. At a certain temperature peculiar to each body, the amplitude of the heat oscillation is sufficient to melt the solid, and we are led to admit that for all bodies the intermolecular distance corresponding to fusion ought to be the same. The higher the point of fusion of a body, the shorter, on this theory, must be its heat vibrations. The product of the length of *swing* (the heat oscillations) by the temperature of fusion ought to be a constant number for all solid bodies.

A comparison of the values of the various quantities involved in these statements shows a very satisfactory agreement between theory and experiment, from which it appears that for many different substances the product of the length of swing by the temperature of fusion lies between 3·3 and 3·7 for most substances. Not many values of the latent heat of dissociation have been made. In order to determine it, say, for the separation of oxygen and hydrogen, we should have to determine the amount of work required to produce a spark in a mixture of oxygen and hydrogen, and to measure the exact amount of water or vapour of water combined by the spark as well as the range of temperature through which it had passed after its formation. Very few such determinations have been made.

Our usual mode of producing heat is by the combination of the molecules of different substances, and we are limited in the production of high temperatures, and in the quantity of available heat necessary to dissociate any considerable quantity of matter. If we heat vapours or gases, we may raise their temperatures up to a point corresponding to the dissociation of their molecules, and we are limited in our chemical actions to the temperatures which can be obtained by combining together the most refractory substances, as we are dependent on this combination for our supply of heat.

The combination of carbon and hydrogen with oxygen will give us high temperatures, so that by the oxyhdrogen blow-pipe most of the salts and oxides are dissociated. The metalloids bromine, iodine, sulphur, potassium, &c., are the results of the combination of two or more bodies bound together by internal forces much stronger than the affinity of hydrogen or carbon for oxygen, for approximately they obey the law of Dulong and Petit.

For higher temperatures, in order to dissociate the most refractory substances, we require the electric current, either a continuous current, as in the electric arc from a battery or a dynamo-machine, or, more intense still, the electrical discharges from an electrical machine or from an induction coil.

This electric current may be regarded as the most intense furnace for dissociating *large* quantities of the most refractory substances, and the electric spark may be regarded as something very much hotter than the oxyhydrogen blow-pipe, and therefore of service in reducing very small quantities of substances which will yield to no other treatment. The temperature of the electric arc is limited, and cannot reach above the temperature of dissociation of the conductor, and in the case of the constant current, which will not leap across the smallest space of air unless the carbons have first been brought in contact, the current very soon ceases when the point of fusion has been reached. Yet in the centre of the arc we have the gases of those substances which form the conductor; and, as Prof. Dewar has shown, we have the formation of acetylene and cyanogen and other compounds, and therefore must have attained the temperature necessary for their formation, *i.e.* the temperature of their dissociation. The temperature of the induction spark, or, at least, its dissociating power, is higher than that of the arc. We know that the spark will pass across a space of air or a gaseous conductor, and we are limited by the dissociation of the gaseous conductor, and get only very small quantities of the dissociated substances, which immediately recombine, unless they are separated. If the gases formed are of different densities they will diffuse at different rates through a porous diaphragm, and so may be obtained separated from one another. As the molecules of bodies vibrate they produce vibrations of the ether particles, the period of the oscillations depends on the molecules of the body, and these periodic vibrations are taken up by their ether envelopes and by the luminiferous ether, and their wave-length determined by means of the spectroscope. The bright-line spectrum may be regarded as arising from the vibratory motions of the atoms. As the temperature is increased, the amplitudes of oscillation of the molecules and of the ether increase, and from the calculations of Lecoq de Boisbaudran, Stoney, Soret, and others, it would appear that many of the lines in the spectra of bodies may be regarded as harmonics of a fundamental vibration. Thus Lecoq de Boisbaudran finds that in the nitrogen spectrum the blue lines seen at a high temperature correspond to the double octave of certain vibrations, and that, at a lower temperature, red and yellow lines are seen which correspond to a fifth of the same fundamental vibrations.

The bright-line spectrum may be regarded as arising from the vibratory motions of the atoms. A widening of the lines may be produced at a higher temperature by the backward and forward motions of the molecules in the direction of the observer. A widening of the lines may also be produced by increase of pressure, because it diminishes the free path of the molecules, and the disturbances of the ether arising from collisions become more important than vibrations arising from the regular vibrations of the atoms.

Band spectra, or chanelled space spectra, more readily occur in the case of bodies which are not very readily subject to chemical actions, or, according to Professors Liveing and Dewar, in the case of cooler vapours near the point of liquefaction.

The effects of change of temperature on the character of spectra is very well illustrated by an experiment of M. Wiedemann with mixtures of mercury with hydrogen or nitrogen in a Geissler's tube. At the ordinary temperature of the air the spectrum of hydrogen or nitrogen was obtained alone; but on heating the tube in an air-bath the lines of mercury appeared and became brighter as the temperature rose, and at the same time the hydrogen lines disappeared in the wider portion of the tube and at the electrodes. The hydrogen or nitrogen lines disappeared first from the positive electrode and in the luminous tuft, and as the temperature rose disappeared altogether. With nitrogen in a particular experiment, up to 100° C., the nitrogen lines were seen throughout the tube, but from 100° to 230° the nitrogen lines appear towards the negative pole, and the mercury lines are less bright at the negative than at the positive pole, while at about 230° C. no nitrogen lines appear. The experiments of Roscoe and Schuster, of Lockyer and other observers, with potassium, sodium, and other metalloids in vacuum tubes, from which hydrogen is pumped by a Sprengel pump, also show great changes in the molecular condition of the mixture contained in the tubes when they are heated to different temperatures. The changes of colour in the tube are accompanied by changes in the spectrum. Thus, Lockyer finds that when potassium is placed in the bottom of the tube, and the spark passes in the upper part of it, as the exhaustion proceeds and the tube is slightly heated, the hydrogen lines disappear, and the red potassium line makes its appearance; then as the temperature is increased, the red line disappears, and three lines in the yellowish-green make their appearance, accompanied by a change in the colour of the tube, and at a higher temperature, and with a Leyden jar joined to a secondary circuit of the induction coil, the gas in the tube becomes of a dull red colour, and with this change a strong line comes out in the spectrum, more refrangible than the usual red potassium line. In this case, on varying the conditions, we get a variation in the character of the spectrum, and the colours and spectra are different in different parts of the tube. In Lockyer's experiments, at the temperature of the arc obtained from a Siemens' dynamo-machine, great differences appear in different parts of the arc: for instance, with carbon poles in the presence of calcium, the band spectrum of carbon, or the carbon flutings and the lines of calcium, some of them reversed, are seen separated in the same way as mercury and hydrogen, the carbon spectrum appearing near one pole and the calcium near the other, the lines which are strongest near that pole being reversed or absorbed by the quantity of calcium vapour surrounding it. On introducing a metal into the arc, lines appear which are of different intensities at different distances from the poles, others are strong at one pole and entirely absent at or near the other, while some lines appear as broad as half-spindles in the middle of the arc, but are not present near the poles. Thus, the blue line of calcium is visible alone at one pole, the H and K line without the blue lines at the other.

We may probably regard these effects as the result, not of temperature alone, but must take into account that we have powerful electric currents which will act unequally on the molecules of different bodies according as they are more or less electro-positive. It would seem that we have here something analogous to the segregation which is observed in the melting of certain alloys to which I have already referred.

The abundance of material in some parts of the arc surrounding the central portion of it gives rise to reversal of the principal lines in varying thicknesses over the arc and poles, so that bright lines appear without reversal in some regions, and reversals or absorption lines without bright lines in others. The introduction of a substance into the arc gives rise to a flame of great complexity with regard to colour and concentric envelopes, and the spectra of these flames differ in different parts of the arc. Thus in a photograph of the flame given by manganese, the line at wave-length 4234·5 occurs without the triplet near 4030, while in another the triplet is present without the line 4234·5.

The lines which are reversed most readily in the arc are generally those the absorption of which is most developed in the flame; thus the manganese triplet in the violet is reversed in the flame, and the blue calcium line is often seen widened when the H and K lines of calcium are not seen at all. In consequence of the numerous changes in spectra at different temperatures, Mr. Lockyer has advanced the idea that the molecules of elementary matter are continually being more and more broken up as their temperature is increased, and has put forward the hypothesis that the chemical elements with which we are acquainted are not simple bodies, but are themselves compounds of some other more simple substances. This theory is founded on Mr. Lockyer's comparisons of spectra and the maps of Ångström, Thalén, Young, and others, in which there are coincidences of many of the short lines of the spectra of different substances. These short lines are termed basic lines, since they appear to be common to two or more substances. They appear at the highest temperatures when the longest lines of those substances and those which are considered the test of their presence are entirely absent.

Mr. Lockyer draws a distinction between weak lines, which are basic, *i.e.* which would permanently exist at a higher temperature in a more elementary stage, and other weak or short lines which would be more strongly present at a lower temperature, in a more complex stage of the molecules. Thus, in lithium, the red line is a low temperature line, and the yellow is feeble; at a higher temperature, the red line is weak, the yellow comes out more strongly, and the blue line appears; at a higher temperature still, the red line disappears, and the yellow dies away; whilst at the temperature of the sun the violet lithium line is the only one which comes out strongly. These effects are studied by first producing the spectrum of the substance in the Bunsen flames, and observing the changes which are produced on passing a spark through the flame; thus, in magnesium, a wide triplet or set of three lines (5209·8, b^1 and b^2) is changed into a narrow triplet (b^1, b^2, and b^4) of the same character. We have here what some observers regard as a recurrence of the same harmonic relation of the vibrations of the same body at a higher temperature.

If the so-called elements are compounds, they must have been formed at a very high temperature, and as higher and higher temperatures are reached the dissociation of these compound bodies will be effected, and the new line spectra, the real basic lines of those substances which show coincidences, will make their appearance as short lines in the spectra. In accordance with this view, Mr. Lockyer holds that the different layers of the solar atmosphere may be regarded as a series of furnaces, on the hottest of which, A, we have the most elementary forms of matter capable of existing only in its uncombined state; at a higher and cooler level, B, this form of matter may form a compound body, and may no longer exist in a free state at the lower temperature; as the cooler and cooler levels, C, D, and E, are reached, the substances become more and more complex and form different combinations, and their spectra become altered at every stage. Since the successive layers are not at rest, but in a state of disturbance, we may get them somewhat mixed, and the lines at the cooler levels D and E may be associated with the lines of the hotter levels; these would be basic or coincident lines in the spectra of two different compounds which exist at the cooler levels D and E. We might even get lines which are not present in the hottest furnace A coming into existence as the lines of compounds in B or C, and then extending among the lines belonging to more complex compounds which can only exist at a lower temperature, when they might be present as coincident weak lines in the spectra of several compound bodies. Thus Mr. Lockyer regards the calcium lines H and K of the solar spectrum as evidence of different molecular groupings of more elementary bodies. In the electric arc with a weak current the single line 4226 of calcium, which is easily reversed, is much thicker than the two lines H and K; but the three lines are equally thick with a stronger current and are all reversed. With a spark from a large coil and using a condenser the line 4226 disappears, and H and K are strong lines. In the sun, the absorption bands H and K are very broad, but the band 4226 is weak. Prof. Young, in his observation of the lines of the chromosphere, finds that H and K are strongly reversed in every important spot and in solar storms; but the line 4226, so prominent in the arc, was only observed three times in the chromosphere.

One of the most interesting features among the most recent researches in spectrum analysis is the existence of rhythm in the spectra of bodies, as has been shown by MM. Mascart, Cornu, and others, such as the occurrence and repetition of sets of lines, doublets, and triplets in the spectra of different substances and in different parts of the spectrum of the same body. Professors Liveing and Dewar, using the reversed lines in some cases for the more acccurate determination of wave-lengths, have traced out the rhythmical character in the spectra of sodium, potassium, and lithium. They show that the lines of sodium and potassium form groups of four lines each, which recur in a regular sequence,

while lithium gives single lines, which, including the green line, which they show really to belong to lithium, though it was ascribed to cæsium by Thalén, also recur in a similar way. In these three metals the law of recurrence seems to be the same, but the wave-lengths show that the whole series are not simple harmonics of one fundamental, although between some of the terms very simple harmonic relations can be found. Between the lines G and H are two triplets of iron lines, which, according to Mr. Lockyer, do not belong to the same molecular grouping as most of the other lines. In many photographs of the iron spectrum these triplets have appeared almost alone. Also the two triplets are not always in the same relation as to brightness, the more refrangible being barely visible with the spark; combining this with Young's observations, in which some short weak lines near G appear in the chromosphere 30 times, while one of the lines of the less refrangible triplet only appears once, and with the fact that in the solar spectrum the more refrangible triplet is much the more prominent of the two, Mr. Lockyer is led to the conclusion that these two triplets are again due to two distinct molecular groupings.

There is one difficulty which must be taken account of in connection with Mr. Lockyer's theory with regard to the production of successive stages of dissociation by means at our command: (1) by combustion of different substances; (2) by an electric arc, which will probably give different temperatures according as it is produced by different dynamo-electric machines; (3) by the induction spark without; and (4) with a condenser.

At each stage of the process there must be a considerable absorption of heat to produce the change of state, and our supply of heat is limited in the electric arc because of the dissociation of the conductors, and more limited still in quantity in the electric spark or in the discharge through a vacuum tube. Also we should expect a recombination of the dissociated substances immediately after they have been first dissociated. Hence it seems easier to suppose that at temperatures which we can command on the earth, the dissociation of molecules by the arc or the spark is accompanied by the formation of new compounds, in the formation of which heat and light, and especially chemical vibrations, would be again given out, thus giving rise to new spectra, rather than to suppose that we can reach the temperature necessary for successive stages of dissociation.

To the lines C, F, the line near G, and *h* belonging to hydrogen, which have a certain rhythmical character, Mr. Lockyer adds D_3 and Kirchhoff's line "1474," regarding "1474" as belonging to the coolest or most complex form, rising to F at a higher temperature, which is again subdivided into C and G, using the spark without a condenser, which again gives *h* with the spark and condenser, which is again split up and gives D_3, a more simple line than *h*, in the Chromosphere. Professors Liveing and Dewar, on the other hand, trace a rhythmical character or ratio between three of the brightest lines of the Chromosphere, two of which are lines "1474" and "f" of Lorenzoni similar to the character of C, F, and *h* of hydrogen, and also trace a similar relation between the chromospheric line D_3 and "1474" to the ratio of the wave-lengths of F and the line near G. They infer the probability that these four lines are due to the same at present unknown substance, as had been suggested by Young with regard to two of them. The harmony of this arrangement is somewhat disturbed by the fact that D_3 lies on the wrong side of "1474" to correspond with the line near G of the hydrogen spectrum.

If we inquire what our sun and the stars have to say to these changes of spectra of the same substance at different temperatures, Dr. Huggins gives us the answer.

In the stars which give a very *white* light, such as Sirius or α Lyræ, we have the lines G and *h* of hydrogen and also H, which has been shown by Dr. Vogel to be coincident with a line of hydrogen; but the K line of calcium is weak in α Lyræ, and does not appear in Sirius. In passing from the white or hottest stars to the yellow stars like our sun, the typical lines diminish in breadth and are better defined, and K becomes stronger relatively to H, and other lines appear. In Arcturus we have a star which is probably cooler than our sun, and in it the line K is stronger in relation to H than it is in the solar spectrum, both being very strong compared with their state in the solar spectrum.

Professors Liveing and Dewar find that K is more easily reversed than H in the electric arc, which agrees with the idea that this line is produced at a lower temperature than H.

Besides the absence or weakness of K, the white stars have twelve strong lines winged at the edges, in which there are three of hydrogen, viz. G, *h*, and H, and the remaining nine form a group which are so related to one another that Dr. Huggins concludes they probably belong to one substance. Three of these lines are said by Dr. Vogel to be lines of hydrogen. Liveing and Dewar have made considerable progress in determining the conditions and the order of reversal of the spectral lines of metallic vapours. They have adopted methods which allow them to observe through greater thicknesses of vapour than previous observers have generally employed. For lower temperatures tubes of iron or other material placed vertically in a furnace were used, and the hot bottom of the tube was the source of light, the absorption being produced by vapours of metals dropped into the hot tube and filling it to a greater or less height. By this means many of the more volatile metals, such as sodium, thallium, iridium, cæsium, and rubidium, magnesium, lithium, barium, strontium, and calcium, each gave a reversal of its most characteristic line or pair of lines, *i.e.* the red line of lithium, the violet lines of rubidium and calcium, the blue line of strontium, the sharp green line of barium (5535), and no other lines which can certainly be ascribed to those metals in the elementary state.

For higher temperatures tubes bored out in blocks of lime or of gas carbon, and heated by the electric arc, were used. By keeping up a supply of metal and in some cases assisting its volatilisation by the admixture of a more volatile metal, such as magnesium, and its reduction by some easily oxidisable metal, such as aluminium, or by a current of coal gas or hydrogen, they succeeded in maintaining a stream of vapour through the tube so as to reverse a great many lines. In this way the greater part of the bright lines of the metals of the alkalies and alkaline earths were reversed, as well as some of the strongest lines of manganese, aluminium, zinc, cadmium, silver, copper, bismuth, and the two characteristic lines of iridium and of gallium. By passing an iron wire into the arc through a perforated carbon electrode they succeeded in obtaining the reversal of many of the lines of iron. In observing bright-line spectra they have found that the arc produced by a De Meritens machine arranged for high tenison gives, in an atmosphere of hydrogen, the lines C and F, although the arc of a powerful Siemens machine does not bring them out, and they have observed many metallic lines in the arc which had not been previously noticed. The temperature obtained by the De Meritens machine is thus higher than that obtained in the Siemens machine.

From observations on weighed quantities of sodium, alone and as an amalgam, introduced into a hot bottle of platinum filled with nitrogen, of which the pressure was varied by an air-pump, they conclude that the width of the sodium lines depends rather on the thickness and temperature of the vapour than upon the whole quantity of sodium present. Very minute quantities diffused into the cool part of the tube gave a broad diffuse absorption, while a thin layer of compressed vapour in the hot part of the tube give only narrow absorption lines. Professors Liveing and Dewar have observed the reversal of some of the well-known bands of the oxides and chlorides of the alkaline earth metals. The lines produced by magnesium in hydrogen form a rhythmical series extending all across the well-known B group, having a close resemblance in general character to the series of lines produced by an electric discharge in a vacuum tube of olefiant gas.

The series appears at all temperatures except when a large condenser is employed along with the induction coil, provided hydrogen is present as well as magnesium, while they disappear when hydrogen is excluded, and never appear in dry nitrogen or carbonic oxide.

From their experiments on carbon spectra they conclude with Ångström and Thalén that certain of the so-called "carbon bands" are due to some compound of carbon with hydrogen, probably acetylene, and that certain others are due to a compound of carbon with nitrogen, probably cyanogen.

They describe some ultra-violet bands: one of them coincides with the shaded band P of the solar spectrum which accompanies the other violet bands in the flame of cyanogen as well as in the arc and spark between carbon electrodes in the nitrogen. All the bands which they ascribe to a compound of carbon and nitrogen disappear when the discharge is taken in a non-nitrogenous gas, and they reappear on the introduction of a minute quantity of nitrogen.

They appear in the flame of hydrocyanic acid, or of cyanogen, even when cooled down as much as possible as shown by Watts,

or when raised to the highest temperature by burning the cyanogen in nitric oxide; but no flames appear to give these bands unless the burning substance contains nitrogen already united with carbon. As the views of Mr. Lockyer with regard to the multiple spectre of carbon have very recently appeared in the pages of NATURE, I need only say that these spectra are looked upon as supporting his theory that the different flutings are truly due to carbon, and that they represent the vibrations of different molecular groupings. The matter is one of very great interest as regards the spectra of comets, for the bands ascribed to acetylene occur in the spectra of comets without the bands of nitrogen, showing that either hydrocarbons must exist ready formed in the comets, in which case the temperature need not exceed that of an ordinary flame, or else nitrogen must be absent, as the temperature which would produce acetylene from its elements would also produce cyanogen, if nitrogen were present.

Quite recently, Professors Liveing and Dewar have, simultaneously with Dr. Huggins, described an ultra-violet emission spectrum of water, and have given maps of this spectrum. It is not a little remarkable that by independent methods these observers should have deduced the same numbers for the wave-lengths of the two strong lines at the most refrangible end of this spectrum.

Great attention has been paid by M. Mascart and by M. Cornu to the ultra-violet end of the solar spectrum. M. Mascart was able to fix lines in the solar spectrum as far as the line R (3179), but was stopped by the faintness of the photographic impression. Prof. Cornu has extended the spectrum still farther to the limit (2948), beyond which no further effect is produced, owing to complete absorption by the earth's atmosphere. A quartz-reflecting prism was used instead of a heliostat. The curvature of the quartz lens was calculated so as to give minimum aberration for a large field of view. The Iceland spar prism was very carefully cut. A lens of quartz was employed to focus the sun on the slit. Having photographed as far as possible by direct solar light, Prof. Cornu compared the solar spectrum directly by means of a fluorescent eye-piece with the spectrum of iron, and then obtained, by photography, the exact positions of the iron lines which were coincident with observed lines in the solar spectrum. M. Cornu states that the dark absorption lines in the sun and the bright iron lines of the same refrangibility are of the same relative importance or intensity in their spectra, indicating the equality between the emissive and the absorbing powers of metallic vapours; and he thinks that we may get by the comparison of bright spectra with the sun some rough approximation to the quantity of metallic vapours present in the absorption layers of the sun's atmosphere. He draws attention to the abundance of the magnetic metals—iron, nickel, and magnesium—and to the fact that these substances form the composition of most meteorites. M. Cornu has studied the extent of the ultra-violet end of the spectrum, and finds that it is more extended in winter than in summer, and that, at different elevations, the gain in length of the spectrum for increase of elevation is very slow, on account of atmospheric absorption, so that we cannot hope greatly to extend the spectrum by taking elevated observing stations. The limit of the solar spectrum is reached very rapidly, and the spectrum is sharply and completely cut off at about the line U (wave-length 2948). From photographs taken at Viesch in the valley of the Rhone, and at the Riffelberg, 1910 metres above it, M. Cornu finds the limits to be at wave-lengths 2950 and 2930 respectively.

In the actual absorption of bright-line spectra by the earth's atmosphere, M. Cornu observed among others three bright-lines of aluminium, which M. Soret calls 30, 31, and 32 (wave-lengths about 1988, 1930, and 1860), and he found that 32 could not be seen at the distance of 6 metres; but on using a collimator and reducing the distance to $1\frac{1}{2}$ metres, the line 32 became visible, notwithstanding the absorption of the extra lens; at 1 metre, line 32 was brighter than 31, and at a quarter of a metre 32 was brighter than either 30 or 31. With a tube 4 metres in length between the collimator and prism ray 32 is not seen; but when the tube is exhausted, ray 31 gains in intensity and 32 comes into view, and gradually gets brighter than 31, whilst 30 changes very little during the exhaustion. With the same tube he found no appreciable difference between the absorption by air very carefully dried and by moist air, and concludes that this absorption is not due to the vapour of water, and it follows the law of pressure of the atmosphere which shows it to be due to the whole mass or thickness of the air. Also, M. Soret has shown that water acts very differently on the two ends of the spectrum, distilled water being perfectly transparent for the most refrangible rays, since a column of water of 116 c.m. allowed the ray 2060 in the spectrum of zinc to pass through; on the other hand, water is so opaque to the ultra-red rays that a length of 1 c.m. of it reduces the heat spectra of metals to half their length and one quarter of their intensity.

In concluding my address, I wish to draw attention to some of those magnetic changes which are due to the action of the Sun, and which are brought about by means of the ether which brings to us his radiant heat and light. In his discussion of the magnetic effects observed on the earth's surface, General Sabine has shown the existence of diurnal variations due to the magnetic action of the sun; also the magnetic disturbances, aurora and earth-currents, which are now again beginning to be large and frequent, have been set down to disturbances in the sun.

Although iron, when raised to incandescence, has its power of attracting a magnet very greatly diminished, we have no proof that it has absolutely no magnetic power left, and with a slight magnetic action the quantity of iron in the sun would be sufficient to account for the diurnal variations of the magnetic needle. During the last few weeks I have been engaged in examining the declination curves for the month of March, 1879, which have been kindly lent to the Kew Committee by the directors of the Observatories of St. Petersburg, Vienna, Lisbon, Coimbra, and Stonyhurst. Other curves are on their way from more distant stations, but have not yet been examined. On comparing them with the Kew curves for the same period, I find the most remarkable coincidences between the curves from these widely-distant stations. It was previously known that there was a similarity between disturbances at different stations, and in one or two cases a comparison between Lisbon and Kew had been made many years ago by Señor Capello and Prof. Balfour Stewart, but the actual photographic magnetic records from several stations have never been previously collected, and so the opportunity for such comparisons had not arisen. Allow me to draw attention to a few of the more prominent features of these comparisons which I have made. On placing the declination curves over one another, I find that in many cases there is absolute agreement between them, so that the rate of change of magnetic disturbances at widely-distant stations like Kew, Vienna, and St. Petersburg, is precisely the same; also similar disturbances take place at different stations at the same absolute time. It may be stated generally, for large as well as small disturbances, that the east and west deflections of the declination needle take place at the same time and are of the same character at these widely-distant stations.

There are exceptions to this law. Some disturbances occur at one or two stations and are not perceived at another station. Many instances occur, where, up to a certain point of time, the disturbances at all the stations are precisely alike, but suddenly at one or two stations the disturbance changes its character: for instance, on comparing Kew and St. Petersburg, we get perfect similarity followed by deflections of the needle opposite ways at the same instant, and in some such cases the maxima in opposite directions are reached at the same instant, showing that the opposite deflections are produced by the same cause, and that the immediate cause or medium of disturbance in such a case is not far off; probably it is some change of direction or intensity of the earth's magnetism arising from solar action upon it.

Generally, after an hour or two, these differences in the effects of the disturbance vanish, and the disturbances again become alike and simultaneous. In such cases of difference, if the curve tracing of the horizontal or the vertical force be examined, it is generally found that, at the very same instant of absolute time, with the beginning of these opposite movements there was an increase or a diminution in the horizontal force, and that the horizontal force continues to change as long as there is any difference in the character of the declination curves. It is clear then from these effects that the cause or causes of magnetic disturbances are in general far distant from the earth's surface, even when those disturbances are large; but that not unfrequently these causes act on magnetic matter nearer to the surface of the earth, and therefore at times between two places of observation, and nearer to one than another, thus producing opposite effects on the declination needle at those places; in such cases the differences are probably due to changes in the earth's magnetic force. Now,

if we imagine the masses of iron, nickel, and magnesium in the sun to retain even a slight degree of magnetic power in their gaseous state—and we know from the researches of Faraday that gases are some of them magnetic—we have a sufficient cause for all our terrestrial magnetic changes, for we know that these masses of metal are ever boiling up from the lower and hotter levels of the sun's atmosphere to the cooler upper regions, where they must again form clouds to throw out their light and heat, and to absorb the light and heat coming from the hotter lower regions; then they become condensed and are drawn again back towards the body of the sun, so forming those remarkable dark spaces or sun-spots by their downrush towards the lower levels.

In these vast changes, which we know from the science of energy must be taking place, but of the vastness of which we can have no conception, we have abundant cause for these magnetic changes which we observe at the same instant at distant points on the surface of the earth, and the same cause acting by induction on the magnetic matter within and on the earth may well produce changes in the magnitude or in the direction of its total magnetic force. These magnetic changes on the earth will influence the declination needles at different places, and will cause them to be deflected; the direction of the deflection must depend on the situation of the earth's magnetic axis or the direction of its motion with regard to the stations where the observations are made. Thus both directly and indirectly we may find in the Sun not only the cause of diurnal magnetic variations, but also the cause of these remarkable magnetic changes and disturbances over the surface of the Earth.

SECTION D

BIOLOGY

Department of Anatomy and Physiology

ADDRESS BY F. M. BALFOUR, M.A., F.R.S., VICE-PRESIDENT OF THE SECTION

IN the spring of the present year Prof. Huxley delivered an address at the Royal Institution, to which he gave the felicitous title of "The Coming of Age of the Origin of Species." It is, as he pointed out, twenty-one years since Mr. Darwin's great work was published, and the present occasion is an appropriate one to review the effect which it has had on the progress of biological knowledge.

There is, I may venture to say, no department of biology the growth of which has not been profoundly influenced by the Darwinian theory. When Messrs. Darwin and Wallace first enunciated their views to the scientific world, the facts they brought forward seemed to many naturalists insufficient to substantiate their far-reaching conclusions. Since that time an overwhelming mass of evidence has, however, been rapidly accumulating in their favour. Facts which at first appeared to be opposed to their theories have one by one been shown to afford striking proofs of their truth. There are at the present time but few naturalists who do not accept in the main the Darwinian theory, and even some of those who reject many of Darwin's explanations still accept the fundamental position that all animals are descended from a common stock.

To attempt in the brief time which I have at my disposal to trace the influence of the Darwinian theory on all the branches of anatomy and physiology would be wholly impossible, and I shall confine myself to an attempt to do so for a small section only. There is perhaps no department of biology which has been so revolutionised, if I may use the term, by the theory of animal evolution as that of Development or Embryology. The reason of this is not far to seek. According to the Darwinian theory the present order of the organic world has been caused by the action of two laws, known as the laws of heredity and of variation. The law of heredity is familiarly exemplified by the well-known fact that offspring resemble their parents. Not only, however, do the offspring belong to the same species as their parents, but they inherit the individual peculiarities of their parents. It is on this that the breeders of cattle depend, and it is a fact of every-day experience amongst ourselves. A further point with reference to heredity to which I must call your attention is the fact that the characters, which display themselves at some special period in the life of the parent, are acquired by the offspring at a corresponding period. Thus, in many birds the males have a special plumage in the adult state. The male offspring is not, however, born with the adult plumage, but only acquires it when it becomes adult.

The law of variation is in a certain sense opposed to the law of heredity. It asserts that the resemblance which offspring bear to their parents is never exact. The contradiction between the two laws is only apparent. All variations and modifications in an organism are directly or indirectly due to its environments; that is to say, they are either produced by some direct influence acting upon the organism itself, or by some more subtle and mysterious action on its parents; and the law of heredity really asserts that the offspring and parent would resemble each other if their environments were the same. Since, however, this is never the case, the offspring always differ to some extent from the parents. Now, according to the law of heredity, every acquired variation tends to be inherited, so that, by a summation of small changes, the animals may come to differ from their parent stock to an indefinite extent.

We are now in a position to follow out the consequences of these two laws in their bearing on development. Their application will best be made apparent by taking a concrete example. Let us suppose a spot on the surface of some very simple organism to become, at a certain period of life, pigmented, and therefore to be especially sensitive to light. In the offspring of this form the pigment-spot will reappear at a corresponding period; and there will therefore be a period in the life of the offspring during which there is no pigment-spot, and a second period in which there is one. If a naturalist were to study the life-history, or, in other words, the embryology, of this form, this fact about the pigment-spot would come to his notice, and he would be justified, from the laws of heredity, in concluding that the species was descended from an ancestor without a pigment-spot, because a pigment-spot was absent in the young. Now, we may suppose the transparent layer of skin above the pigment-spot to become thickened, so as gradually to form a kind of lens, which would throw an image of external objects on the pigment-spot. In this way a rudimentary eye might be evolved out of the pigment-spot. A naturalist studying the embryology of the form with this eye would find that the pigment-spot was formed before the lens, and he would be justified in concluding, by the same process of reasoning as before, that the ancestors of the form he was studying first acquired a pigment-spot and then a lens. We may picture to ourselves a series of steps by which the simple eye, the origin of which I have traced, might become more complicated; and it is easy to see how an embryologist studying the actual development of this complicated eye would be able to unravel the process of its evolution.

The general nature of the methods of reasoning employed by embryologists, who accept the Darwinian theory, is exemplified by the instance just given. If this method is a legitimate one, and there is no reason to doubt it, we ought to find that animals, in the course of their development, pass through a series of stages, in each of which they resemble one of their remote ancestors; but it is to be remembered that, in accordance with the law of variation, there is a continual tendency to change, and that the longer this tendency acts the greater will be the total effect. Owing to this tendency we should not expect to find a perfect resemblance between an animal, at different stages of its growth, and its ancestors; and the remoter the ancestors, the less close ought the resemblance to be. In spite, however, of this limitation, it may be laid down as one of the consequences of the law of inheritance that every animal ought, in the course of its individual development, to repeat with more or less fidelity the history of its ancestral evolution.

A direct verification of this proposition is scarcely possible. There is ample ground for concluding that the forms from which existing animals are descended have in most instances perished; and although there is no reason why they should not have been preserved in a fossil state, yet, owing to the imperfection of the geological record, palæontology is not so often of service as might have been hoped.

While, for the reasons just stated, it is not generally possible to prove by direct observation that existing forms in their embryonic state repeat the characters of their ancestors, there is another method by which the truth of this proposition can be approximately verified.

A comparison of recent and fossil forms shows that there are actually living at the present day representatives of a considerable proportion of the groups which have in previous times existed on the globe, and there are therefore forms allied to the ancestors of those living at the present day, though not actually

the same species. If therefore it can be shown that the embryos of existing forms pass through stages in which they have the characters of more primitive groups, a sufficient proof of our proposition will have been given.

That such is often the case is a well-known fact, and was even known before the publication of Darwin's works. Von Baer, the greatest embryologist of the century, who died at an advanced age but a few years ago, discussed the proposition at considerable length in a work published between the years 1830 and 1840. He came to the conclusion that the embryos of higher forms never actually resemble lower forms, but only the embryos of lower forms; and he further maintained that such resemblances did not hold at all, or only to a very small extent, beyond the limits of the larger groups. Thus he believed that, though the embryos of Vertebrates might agree amongst themselves, there was no resemblance between them and the embryos of any invertebrate group. We now know that these limitations of von Baer do not hold good, but it is to be remembered that the meaning *now* attached by embryologists to such resemblances was quite unknown to him.

These preliminary remarks will, I trust, be sufficient to demonstrate how completely modern embryological reasoning is dependent on the two laws of inheritance and variation, which constitute the keystones of the Darwinian theory.

Before the appearance of the "Origin of Species" many very valuable embryological investigations were made, but the facts discovered were to their authors merely so many ultimate facts, which admitted of being classified, but could not be explained. No explanation could be offered of why it is that animals, instead of developing in a simple and straightforward way, undergo in the course of their growth a series of complicated changes, during which they often acquire organs which have no function, and which, after remaining visible for a short time, disappear without leaving a trace.

No explanation, for instance, could be offered of why it is that a frog in the course of its growth has a stage in which it breathes like a fish, and then why it is like a newt with a long tail, which gradually becomes absorbed, and finally disappears. To the Darwinian the explanation of such facts is obvious. The stage when the tadpole breathes by gills is a repetition of the stage when the ancestors of the frog had not advanced in the scale of development beyond a fish, while the newt-like stage implies that the ancestors of the frog were at one time organised very much like the newts of to-day. The explanation of such facts has opened out to the embryologist quite a new series of problems. These problems may be divided into two main groups, technically known as those of phylogeny and those of organogeny. The problems of phylogeny deal with the genealogy of the animal kingdom. A complete genealogy would form what is known as a natural classification. To attempt to form such a classification has long been the aim of a large number of naturalists, and it has frequently been attempted without the aid of embryology. The statements made in the earlier part of my address clearly show how great an assistance embryology is capable of giving in phylogeny; and as a matter of fact embryology has been during the last few years very widely employed in all phylogenetic questions, and the results which have been arrived at have in many cases been very striking. To deal with these results in detail would lead me into too technical a department of my subject; but I may point out that amongst the more striking of the results obtained *entirely* by embryological methods is the demonstration that the Vertebrata are not, as was nearly universally believed by older naturalists, separated by a wide gulf from the Invertebrata, but that there is a group of animals, known as the Ascidians, formerly united with the Invertebrata, which are now universally placed with the Vertebrata.

The discoveries recently made in organogeny, or the genesis of organs, have been quite as striking, and in many respects even more interesting, than those in phylogeny, and I propose devoting the remainder of my address to a history of results which have been arrived at with reference to the origin of the nervous system.

To render clear the nature of these results I must say a few words as to the structure of the animal body. The body is always built of certain pieces of protoplasm, which are technically known to biologists as cells. The simplest organisms are composed either of a single piece of this kind, or of several similar pieces loosely aggregated together. Each of these pieces or cells is capable of digesting and assimilating food, and of respiring; it can execute movements, and is sensitive to external stimuli, and can reproduce itself. All the functions of higher animals can, in fact, be carried on in this single cell. Such lowly-organised forms are known to naturalists as the Protozoa. All other animals are also composed of cells, but these cells are no longer complete organisms in themselves. They exhibit a division of labour: some carrying on the work of digestion; some, which we call nerve-cells, receiving and conducting stimuli; some, which we call muscle-cells, altering their form—in fact, contracting in one direction—under the action of the stimuli brought to them by the nerve-cells. In most cases a number of cells with the same function are united together, and thus constitute a tissue. Thus the cells which carry on the work of digestion form a lining membrane to a tube or sac, and constitute a tissue known as a secretory epithelium. The whole of the animals with bodies composed of definite tissues of this kind are known as the Metazoa.

A considerable number of early developmental processes are common to the whole of the Metazoa.

In the first place every Metazoon commences its existence as a simple cell, in the sense above defined; this cell is known as the ovum. The first developmental process which takes place consists in the division or segmentation of the single cell into a number of smaller cells. The cells then arrange themselves into two groups or layers known to embryologists as the *primary germinal layers*. These two layers are usually placed one within the other round a central cavity. The inner of the two is called the hypoblast, the outer the epiblast. The existence of these two layers in the embryos of vertebrated animals was made out early in the present century by Pander, and his observations were greatly extended by von Baer and Remak. But it was supposed that these layers were confined to vertebrated animals. In the year 1849, and at greater length in 1859, Huxley demonstrated that the bodies of all the polype tribe or Cœlenterata—that is to say, of the group to which the common polype, jelly-fish, and the sea-anemone belong—were composed of two layers of cells, and stated that in his opinion these two layers were homologous with the epiblast and hypoblast of vertebrate embryos. This very brilliant discovery came before its time. It fell upon barren ground, and for a long time bore no fruit. In the year 1860 a young Russian naturalist named Kowalevsky began to study by special histological methods the development of a number of invertebrated forms of animals, and discovered that at an early stage of development the bodies of all these animals were divided into germinal layers like those in vertebrates. Biologists were not long in recognising the importance of these discoveries, and they formed the basis of two remarkable essays, one by our own countryman, Prof. Lankester, and the other by a distinguished German naturalist, Prof. Haeckel of Jena.

In these essays the attempt was made to show that the stage in development already spoken of, in which the cells are arranged in the form of two layers inclosing a central cavity, has an ancestral meaning, and that it is to be interpreted to signify that all the Metazoa are descended from an ancestor which had a more or less oval form, with a central digestive cavity provided with a single opening, serving both for the introduction of food and for the ejection of indigestible substances. The body of this ancestor was supposed to have been a double-walled sac formed of an inner layer, the hypoblast, lining the digestive cavity, and an outer layer, the epiblast. To this form Haeckel gave the name of gastræa or gastrula.

There is every reason to think that Lankester and Haeckel were quite justified in concluding that a form more or less like that just described was the ancestor of the Metazoa; but the further speculations contained in their essays as to the origin of this form from the Protozoa can only be regarded as suggestive feelers, which, however, have been of great importance in stimulating and directing embryological research. It is, moreover, very doubtful whether there are to be found in the developmental histories of most animals any traces of this gastræa ancestor, other than the fact of their passing through a stage in which the cells are divided into two germinal layers.

The key to the nature of the two germinal layers is to be found in Huxley's comparison between them, and the two layers in the fresh-water polype and the sea-anemone. The epiblast is the primitive skin, and the hypoblast is the primitive epithelial wall of the alimentary tract.

In the whole of the polype group, or Cœlenterata, the body remains through life composed of the two layers, which Huxley recognised as homologous with the epiblast and hypoblast of the Vertebrata; but in all the higher Metazoa a third germinal

layer, known as the mesoblast, early makes its appearance between the two primary layers. The mesoblast originates as a differentiation of one or of both the primary germinal layers; but although the different views which have been held as to its mode of origin form an important section of the history of recent embryological investigations, I must for the moment confine myself to saying that from this layer there take their origin—the whole of the muscular system, of the vascular system, and of that connective-tissue system which forms the internal skeleton, tendons, and other parts.

We have seen that the epiblast represents the skin or epidermis of the simple sac-like ancestor common to all the Metazoa. In all the higher Metazoa it gives rise, as might be expected, to the epidermis, but it gives rise at the same time to a number of other organs; and, in accordance with the principles laid down in the earlier part of my address, it is to be concluded that *the organs so derived have been formed as differentiations of the primitive epidermis.* One of the most interesting of recent embryological discoveries is the fact that the nervous system is, in all but a very few doubtful cases, derived from the epiblast. This fact was made out for vertebrate animals by the great embryologist Van Baer; and the Russian naturalist Kowalevsky, to whose researches I have already alluded, showed that this was true for a large number of invertebrate animals. The derivation of the nervous system from the epiblast has since been made out for a sufficient number of forms satisfactorily to establish the generalisation that it is all but universally derived from the epiblast.

In any animal in which there is no distinct nervous system it is obvious that the general surface of the body must be sensitive to the action of its surroundings, or to what are technically called stimuli. We know experimentally that this is so in the case of the Protozoa, and of some very simple Metazoa, such as the fresh-water Polype or Hydra, where there is no distinct nervous system. The skin or epidermis of the ancestor of the Metazoa was no doubt similarly sensitive; and the fact of the nervous system being derived from the epiblast implies that the functions of the central nervous system, which were originally taken by the whole skin, became gradually concentrated in a special part of the skin which was step by step removed from the surface, and finally became a well-defined organ in the interior of the body.

What were the steps by which this remarkable process took place? How has it come about that there are nerves passing from the central nervous system to all parts of the skin, and also to the muscles? How have the arrangements for reflex actions arisen by which stimuli received on the surface of the body are carried to the central part of the nervous system, and are thence transmitted to the appropriate muscles, and cause them to contract? All these questions require to be answered before we can be said to possess a satisfactory knowledge of the origin of the nervous system. As yet, however, the knowledge of these points derived from embryology is imperfect, although there is every hope that further investigation will render it less so? Fortunately, however, a study of comparative anatomy, especially that of the Cœlenterata, fills up some of the gaps left from our study of embryology.

From embryology we learn that the ganglion-cells of the central part of the nervous system are originally derived from the simple undifferentiated epithelial cells of the surface of the body. We further learn that the nerves are out-growths of the central nervous system. It was supposed till quite recently that the nerves in Vertebrates were derived from parts of the middle germinal layer or mesoblast, and that they only became secondarily connected with the central nervous system. This is now known not to be the case, but the nerves are formed as processes growing out from the central part of the nervous system.

Another important fact shown by embryology is that the central nervous system, and percipient portion of the organs of special sense, are often formed from the same part of the primitive epidermis. Thus, in ourselves and in other vertebrate animals the sensitive part of the eye, known as the retina, is formed from two lateral lobes of the front part of the primitive brain. The crystalline lens and cornea of the eye are, however, subsequently formed from the skin.

The same is true for the peculiar compound eyes of crabs or Crustacea. The most important part of the central nervous system of these animals is the supracœsophageal ganglia, often known as the brain, and these are formed in the embryo from two thickened patches of the skin at the front end of the body. These thickened patches become gradually detached from the surface, remaining covered over by a layer of skin. They then constitute the supracœsophageal ganglia; but they form not only the ganglia, but also the rhabdoms or retinal elements of the eye—the parts in fact which correspond to the rods and cones in our own retina. The layer of epidermis or skin which lies immediately above the supracœsophageal ganglia becomes gradually converted into the refractive media of the crustacean eye. A cuticle which lies on its surface forms the peculiar facets on the surface of the eye, which are known as the corneal lenses, while the cells of the epidermis give rise to lens-like bodies known as the crystalline cones.

It would be easy to quote further instances of the same kind, but I trust that the two which I have given will be sufficient to show the kind of relation which often exists between the organs of special sense, especially those of vision, and the central nervous system. It might have been anticipated *à priori* that organs of special sense would only appear in animals provided with a well-developed central nervous system. This, however, is not the case. Special cells with long delicate hairs, which are undoubtedly highly sensitive structures, are present in animals in which as yet nothing has been found which could be called a central nervous system; and there is every reason to think that the organs of special sense originated *pari passu* with the central nervous system. It is probable that in the simplest organisms the whole body is sensitive to light, but that with the appearance of pigment-cells in certain parts of the body, the sensitiveness to light became localised to the areas where the pigment-cells were present. Since, however, it was necessary that stimuli received by such organs should be communicated to other parts of the body, some of the epidermic cells in the neighbourhood of the pigment-spots, which were at first only sensitive, in the same manner as other cells of the epidermis, became gradually differentiated into special nerve-cells. As to the details of this differentiation, embryology does not as yet throw any great light; but from the study of comparative anatomy there are grounds for thinking that it was somewhat as follows:—Cells placed on the surface sent protoplasmic processes of a nervous nature inwards, which came into connection with nervous processes from similar cells placed in other parts of the body. The cells with such processes then became removed from the surface, forming a deep layer of the epidermis below the sensitive cells of the organ of vision. With these cells they remained connected by protoplasmic filaments, and thus they came to form a thickening of the epidermis underneath the organ of vision, the cells of which received their stimuli from those of the organ of vision, and transmitted the stimuli so received to other parts of the body. Such a thickening would obviously be the rudiment of a central nervous system, and it is easy to see by what steps it might become gradually larger and more important, and might gradually travel inwards, remaining connected with the sense-organ at the surface by protoplasmic filaments, which would then constitute nerves. The rudimentary eye would at first merely consist partly of cells sensitive to light, and partly of optical structures constituting the lens, which would throw an image of external objects upon it, and so convert the whole structure into a true organ of vision. It has thus come about that, in the development of the individual, the retina or sensitive part of the eye is first formed in connection with the central nervous system, while the lenses of the eye are independently evolved from the epidermis at a later period.

The general features of the origin of the nervous system which have so far been made out by means of the study of embryology are the following:—

1. That the nervous system of the higher Metazoa has been developed in the course of a long series of generations by a gradual process of differentiation of parts of the epidermis.

2. That part of the central nervous system of many forms arose as a local collection of nerve-cells in the epidermis, in the neighbourhood of rudimentary organs of vision.

3. That ganglion-cells have been evolved from simple epithelial cells of the epidermis.

4. That the primitive nerves were outgrowths of the original ganglion cells; and that the nerves of the higher forms are formed as outgrowths of the central nervous system.

The points on which embryology has not yet thrown a satisfactory light are:—

1. The steps by which the protoplasmic processes, from the primitive epidermic cells, became united together so as to form

a network of nerve-fibres, placing the various parts of the body in nervous communication.

2. The process by which nerves became connected with muscles, so that a stimulus received by a nerve-cell could be communicated to and cause a contraction in a muscle.

Recent investigations on the anatomy of the Cœlenterata, especially of jelly-fish and sea-anemones, have thrown some light on these points, although there is left much that is still obscure.

In our own country Mr. Romanes has conducted some interesting physiological experiments on these forms; and Prof. Schäfer has made some important histological investigations upon them. In Germany a series of interesting researches have also been made on them by Professors Kleinenberg, Claus, and Eimer, and more especially by the brothers Hertwig, of Jena. Careful histological investigations, especially those of the last-named authors, have made us acquainted with the forms of some very primitive types of nervous system. In the common sea-anemones there are, for instance, no organs of special sense, and no definite central nervous system. There are, however, scattered throughout the skin, and also throughout the lining of the digestive tract, a number of specially modified epithelial cells, which are no doubt delicate organs of sense. They are provided at their free extremity with a long hair, and are prolonged on their inner side into a fine process which penetrates the deeper part of the epithelial layer of the skin or digestive wall. They eventually join a fine network of protoplasmic fibres which forms a special layer immediately within the epithelium. The fibres of this network are no doubt essentially nervous. In addition to fibres there are, moreover, present in the network cells of the same character as the multipolar ganglion-cells in the nervous system of Vertebrates, and some of these cells are characterised by sending a process into the superjacent epithelium. Such cells are obviously epithelial cells in the act of becoming nerve-cells; and it is probable that the nerve-cells are, in fact, sense-cells which have travelled inwards and lost their epithelial character.

There is every reason to think that the network just described is not only continuous with the sense-cells in the epithelium, but that it is also continuous with epithelial cells which are provided with muscular prolongations. The nervous system thus consists of a network of protoplasmic fibres, continuous on the one hand with sense-cells in the epithelium, and on the other with muscular cells. The nervous network is generally distributed both beneath the epithelium of the skin and that of the digestive tract, but is especially concentrated in the disk-like region between the mouth and tentacles. The above observations have thrown a very clear light on the characters of the nervous system at an early stage of its evolution, but they leave unanswered the questions (1) how the nervous network first arose, and (2) how its fibres became continuous with muscles. It is probable that the nervous network took its origin from processes of the sense-cells. The processes of the different cells probably first met and then fused together, and becoming more arborescent, finally gave rise to a complicated network.

The connection between this network and the muscular cells also probably took place by a process of contact and fusion.

Epithelial cells with muscular processes were discovered by Kleinenberg before epithelial cells with nervous processes were known, and he suggested that the epithelial part of such cells was a sense-organ, and that the connecting part between this and the contractile processes was a rudimentary nerve. This ingenious theory explained completely the fact of nerves being continuous with muscles; but on the further discoveries being made which I have just described, it became obvious that this theory would have to be abandoned, and that some other explanation would have to be given of the continuity between nerves and muscles. The hypothetical explanation just offered is that of fusion.

It seems very probable that many of the epithelial cells were originally provided with processes, the protoplasm of which, like that of the Protozoa, carried on the functions of nerves and muscles at the same time, and that these processes united amongst themselves into a network. By a process of differentiation parts of this network may have become specially contractile, and other parts may have lost their contractility and become solely nervous. In this way the connection between nerves and muscles might be explained, and this hypothesis fits in very well with the condition of the neuro-muscular system as we find it in the Cœlenterata.

The nervous system of the higher Metazoa appears then to have originated from a differentiation of some of the superficial epithelial cells of the body, though it is possible that some parts of the system may have been formed by a differentiation of the alimentary epithelium. The cells of the epithelium were most likely at the same time contractile and sensory, and the differentiation of the nervous system may very probably have commenced, in the first instance, from a specialisation in the function of part of a network formed of neuro-muscular prolongations of epithelial cells. A simultaneous differentiation of other parts of the network into muscular fibres may have led to the continuity at present obtaining between nerves and muscles.

Local differentiations of the nervous network, which was no doubt distributed over the whole body, took place on the formation of organs of special sense, and such differentiations gave rise to the formation of a central nervous system. The central nervous system was at first continuous with the epidermis, but became separated from it and travelled inwards. Ganglion-cells took their origin from sensory epithelial cells provided with prolongations continuous with the nervous network. Such epithelial cells gradually lost their epithelial character, and finally became completely detached from the epidermis.

Nerves, such as we find them in the higher types, originated from special differentiations of the nervous network, radiating from the parts of the central nervous system.

Such, briefly, is the present state of our knowledge as to the genesis of the nervous system. I ought not, however, to leave this subject without saying a few words as to the hypothetical views which the distinguished evolutionist Mr. Herbert Spencer has put forward on this subject in his work on Psychology.

For Herbert Spencer nerves have originated, not as processes of epithelial cells, but from the passage of motion along the lines of least resistance. The nerves would seem, according to this view, to have been formed in any tissue from the continuous passage of nervous impulses through it. "A wave of molecular disturbance," he says, "passing along a tract of mingled colloids closely allied in composition, and isomerically transforming the molecules of one of them, will be apt at the same time to form some new molecules of the same type," and thus a nerve becomes established.

A nervous centre is formed, according to Herbert Spencer, at the point in the colloid in which nerves are generated, where a single nervous wave breaks up, and its parts diverge along various lines of least resistance. At such points some of the nerve-colloid will remain in an amorphous state, and as the wave of molecular motion will there be checked, it will tend to cause decompositions amongst the unarranged molecules. The decompositions must, he says, cause "additional molecular motion to be disengaged; so that along the outgoing lines there will be discharged an augmented wave. Thus there will arise at this point something having the character of a ganglion corpuscle."

These hypotheses of Herbert Spencer, which have been widely adopted in this country, are, it appears to me, not borne out by the discoveries to which I have called your attention to-day. The discovery that nerves have been developed from processes of epithelial cells, gives a very different conception of their genesis to that of Herbert Spencer, which makes them originate from the passage of nervous impulses through a tract of mingled colloids; while the demonstration that ganglion-cells arose as epithelial cells of special sense, which have travelled inwards from the surface, admits still less of a reconciliation with Herbert Spencer's view on the same subject.

Although the present state of our knowledge on the genesis of the nervous system is a great advance on that of a few years ago, there is still much remaining to be done to make it complete.

The subject is well worth the attention of the morphologist, the physiologist, or even of the psychologist, and we must not remain satisfied by filling up the gaps in our knowledge by such hypotheses as I have been compelled to frame. New methods of research will probably be required to grapple with the problems that are still unsolved; but when we look back and survey what has been done in the past, there can be no reason for mistrusting our advance in the future.

Department of Anthropology

ADDRESS BY F. W. RUDLER, F.G.S., VICE-PRESIDENT OF THE SECTION

AFTER referring to the ethnologically mixed state of the population of South Wales, Mr. Rudler went on—What then

are the ethnical relations of the typical man of South Wales?

Nine people out of every ten to whom this question might be addressed would unhesitatingly answer that the true Welsh are Celts or Kelts.[1] And they would seek to justify their answer by a confident appeal to the Welsh language. No one has any doubt about the position of this language as a member of the Keltic family. The Welsh and the Breton fall naturally together as living members of a group of languages to which Prof. Rhys applies the term *Brythonic*, a group which also includes such fossil tongues as the old Cornish, the speech of the Strathclyde Britons, and possibly the language of the Picts and of the Gauls. On the other hand, the Gaelic of Scotland, the Irish, and the Manx arrange themselves as naturally in another group, which Prof. Rhys distinguishes as the *Goidelic* branch of the Keltic stock.[2] But does it necessarily follow that all the peoples who are closely linked together by speaking, or by having at some time spoken, these Keltic languages, are as closely linked together by ties of blood? Great as the value of language unquestionably is as an aid to ethnological classification, are we quite safe in concluding that all the Keltic-speaking peoples are one in race?

The answer to such a question must needs depend upon the sense in which the anthropologist uses the word Kelt. History and tradition, philology and ethnology, archæology and craniology, have at different times given widely divergent definitions of the term. Sometimes the word has been used with such elasticity as to cover a multitude of peoples who differ so widely one from another in physical characteristics that if the hereditary persistence of such qualities counts for anything, they cannot possibly be referred to a common stock. Sometimes, on the other hand, the word has been so restricted in its definition that it has actually excluded the most typical of all Kelts—the Gaulish Kelts of Cæsar. According to one authority the Kelt is short, according to another tall; one ethnologist defines him as being dark, another as fair; this craniologist finds that he has a long skull, while that one declares that his skull is short. It was no doubt this ambiguity that led so keen an observer as Dr. Beddoe to remark, nearly fifteen years ago, that "Kelt and Keltic are terms which were useful in their day, but which have ceased to convey a distinct idea to the minds of modern students."[3]

No anthropologist has laboured more persistently in endeavouring to evoke order out of this Keltic chaos than the late Paul Broca. What, let us ask, was the opinion of this distinguished anthropologist on the Keltic question?[4] Prof. Broca always held that the name of Kelt should be strictly limited to the Kelt of positive history—to the people, or rather confederation of peoples, actually seen by Cæsar in Keltic Gaul—and, of course, to their descendants in the same area. Every schoolboy is familiar with the epitome of Gaulish ethnology given by Julius in his opening chapter. Nothing can be clearer than his description of the tripartite division of Gaul, and of the separation between the three peoples who inhabited the country—the Belgæ, the Aquitani, and the Celtæ. Of these three peoples the most important were those whom the Romans called *Galli*, but who called themselves, as the historian tells us, *Celtæ*. The country occupied by the Keltic population stretched from the Alps to the Atlantic in one direction, and from the Seine to the Garonne in another; but it is difficult to find any direct evidence that the Kelts of this area ever crossed into Britain. Broca refused to apply the name of Kelt to the old inhabitants of Belgic Gaul, and as a matter of course

[1] Whether this word should be written Celt or Kelt seems to be a matter of scientific indifference. Probably the balance of opinion among ethnologists is in the direction of the former rendering. Nevertheless it must be borne in mind that the word "celt" is so commonly used nowadays by writers on prehistoric anthropology to designate an axe-head, or some such weapon, whether of metal or of stone, that it is obviously desirable to make the difference between the archæological word and the ethnological term as clear as possible. If ethnologists persist in writing "Celt," the two words differ only in the magnitude of an initial, and when spoken are absolutely indistinguishable. I shall therefore write, as a matter of expediency, "Kelt." It may be true, as Mr. Knight Watson has pointed out, that there was originally no justification for using the word "celt" as the name of a weapon, but it is too late in the day to attempt to oust so deeply-rooted a word from the vocabulary of the archæologists.

[2] "Lectures on Welsh Philology," by John Rhys, M.A., 2nd edition, 1878, p. 15.

[3] *Mem.* Anthrop. Soc. Lon., vol. ii 1866, p. 348.

[4] The following are Broca's principal contributions to this vexed question:—"Qu'est-ce que les Celtes?" *Bulletins de la Société Anthropologique de Paris*, t. v. p. 457; "Le Nom des Celtes," *ibid.* 2 sér. t. ix. p. 662; "Sur les Textes relatifs aux Celtes dans le Grande-Bretagne," *ibid.* 2 sér. t. xii. p. 509; "La Race Celtique, ancienne et moderne," *Revue d'Anthropologie*, t. ii. p. 578; and "Recherches sur l'Ethnologie de la France," *Mém.* de la Soc. Anthrop., t. i. p. 1.

he denied it to any of the inhabitants of the British Isles. Writing as late as 1877, in full view of all the arguments which had been adduced against his opinions, he still said: "Je continue à soutenir, jusqu'à preuve du contraire, ce que j'ai avancé il y a douze, ans dans notre première discussion sur les Celtes, savoir, qu'il n'existe aucune preuve, qu'on ait constaté dans les Iles-Britanniques l'existence d'un peuple portant le nom de Celtes."[1]

Nevertheless, in discussing the Keltic question with M. Henri Martin, he admitted the convenience, almost the propriety, of referring to all who spoke Keltic languages as *Keltic* peoples, though of course he would not hear of their being called Kelts. "On peut très bien les nommer les peuples celtiques. Mais il est entièrement faux de les appeler les *Celtes*, comme on le fait si souvent."[2] As to the eminent historian himself, I need hardly say that M. Martin adheres to the popular use of the word Kelt, and even goes so far as to speak of the county in which we are now assembled as "le Glamorgan, le pays aujourd'hui le plus celtique de l'Europe."[3]

Whether we use the word Kelt in its wide linguistic sense or in the narrower sense to which it has been reduced by the French anthropologists, it is important to remember that the Welsh do not designate, and never have designated themselves by this term or by any similar word. Their national name is *Cymry*, the plural of *Cymro*. My former colleague, the Rev. Prof. Silvan Evans, kindly informs me that the most probable derivation of this word is from *cyd-* (the *d* being changed to *m* for assimilation with the following *b*, like the *n* of its Latin cognate *con*) and *bro-*, "country," the old form of which is *brog*, as found in *Allobrogæ*, and some other ancient names. The meaning of *Cymry* is therefore "fellow-countrymen," or compatriots. Such a meaning naturally suggests that the name must have been assumed in consequence of some foreign invasion—possibly when the Welsh were banded together against either the Romans or the English. If this assumption be correct it must be a word of comparatively late origin.

At the same time, the similarity between *Cymry* and *Cimbri*—the name of those dread foes of the Romans whom Marius eventually conquered—is so close as to naturally suggest a common origin for the two names, if not for the people who bore the names.[4] The warlike Cimbri have generally been identified with the people who inhabited the Cimbric peninsula, the *Chersonesus Cimbrica*, now called Jutland. Whether they were connected or not with the *Kimmerioi*, who dwelt in the valley of the Danube and in the Tauric Chersonesus or *Crimea*, is a wider question with which we are not at the moment concerned. As to the ethnical relations of the Cimbri, two views have been current, the one regarding them as of Germanic, the other as of Keltic stock. Canon Rawlinson, in summing up the evidence on both sides, believes that the balance of opinion inclines to the Keltic view.[5] These Cimbri are described, however, as having been tall, blue-eyed, and yellow- or flaxen-haired men. Can we trace anything like these characters in the Cymry?

All the evidence which the ethnologist is able to glean from classical writers with respect to the physical characters and ethnical relations of the ancient inhabitants of this country may be put into a nutshell with room to spare. The exceeding meagreness of our data from this source will be admitted by any one who glances over the passages relating to Britain which are collected in the "Monumenta Historica Britannica." As to the people in the south, there is the well-known statement in Cæsar, that the maritime parts of Britain, the southern parts which he personally visited, were peopled by those who had crossed over from the Belgæ, for what purpose we need not inquire. Of the Britons of the interior, whom he never saw, he merely repeats a popular tradition which represented them as aborigines.[6] They may, therefore, have been Keltic tribes, akin to the Celti of Gaul, though there is nothing in Cæsar's words to support such a view.

Tacitus, in writing the life of his father-in-law, Agricola,

[1] *Bulletins de la Société Anthropologique de Paris*, 2 sér. t. xii. 1878, p. 511.

[2] *Ibid.* t. ix. 1874, p. 662.

[3] *Ibid.*, t. xii. p. 486.

[4] Prof. Rhys, however, has pointed out that there is no relation between the names. See "British Barrows," by Canon Greenwell and Prof. Rolleston, 1877, p. 632.

[5] "On the Ethnography of the Cimbri" By Canon Rawlinson. *Journ.* Anthrop. Inst., vol. vi. 1877, p. 150. See also Dr. Latham's paper and postscript, "On the Evidence of a Connection between the Cimbri and the Chersonesus Cimbrica, published in his "Germania of Tacitus."

[6] "Britanniæ pars interior ab iis incolitur, quos natos in insula ipsi memoria proditum dicunt: maritima pars ab iis, qui prædæ ac belli inferendi causa ex Belgis transierant."—*De Bello Gallico*, lib. v. c. 12.

says that the Britons nearest to Gaul resembled the Gauls.[1] If he refers here to the sea-coast tribes in the south-east of Briton, the comparison must be with the Belgic and not with the Keltic Gauls. But his subsequent reference to the resemblance between the sacred rites of the Britons and those of the Gauls suggests that his remarks may be fairly extended to the inland tribes beyond the limits of the Belgic Britons, in which case the resemblance may be rather with the Gaulish Kelts. Indeed this inference, apart from the testimony of language, is the chief evidence upon which ethnologists have based their conclusions as to the Keltic origin of the Britons.

Our data for restoring the anthropological characteristics of the ancient Britons are but few and small. It is true that a description of Banduica, or Boadicea, has been left to us by Xiphiline, of Trebizond; but then it will be objected that he did not write until the twelfth century. Yet it must be remembered that he merely abridged the works of Dion Cassius, the historian, who wrote a thousand years earlier, and consequently we have grounds for believing that what Xiphiline describes is simply a description taken from the lost books of an early historian who is supposed to have drawn his information from original sources. Now Boadicea is described in these terms: "She was of the largest size, most terrible of aspect, most savage of countenance, and harsh of voice, having a profusion of yellow hair which fell down to her hips."[2] Making due allowance for rhetorical exaggeration, making allowance too for the fact that in consequence of her royal descent she is likely to have been above the average stature, and even admitting that she dyed her hair, it is yet clear that this British queen must be regarded as belonging to the xanthous type—tall and fair. The tribe of the Iceni, over which this blonde amazon ruled, is generally placed beyond the limits of the Belgic Britons; though some authorities have argued in favour of a Belgic origin. If the latter view be correct, we should expect the queen to be tall, light-haired, and blue-eyed; for, from what we know of the Belgæ, such were their features. Cæsar asserts that the majority of the Belgæ were derived from the Germans.[3] But notwithstanding this assertion most ethnologists are inclined to ally them with the Celti, without, of course, denying a strong Teutonic admixture. Strabo says[4] that the Belgæ and Celti had the same Gaulish form, though both differed widely in physical characters from the Aquitanians. As to language, Cæsar's statement that the Belgic and Keltic differed, probably refers only to dialectical differences.[5] If a close ethnical relationship can be established between the Celti and the Belgæ, British ethnology clearly gains in simplification. To what extent the Belgic settlers in this country resembled the neighbouring British tribes must remain a moot point. According to Strabo[6] the Britons were taller than the Celti, with hair less yellow, and they were slighter in build. By the French school of ethnologists the Belgæ are identified with the Cymry, and are described as a tall fair people, similar to the Cimbri already mentioned; and Dr. Pritchard, the founder of English anthropology, was led long ago to describe the Keltic type in similar terms.[7]

Yet as we pass across Britain westwards, and advance towards those parts which are reputed to be predominantly Keltic, the proportion of tall fair folk, speaking in general terms, diminishes, while the short and dark element in the population increases, until it probably attains its maximum somewhere in this district. As popular impressions are apt to lead us astray, let us turn for accuracy to the valuable mass of statistics collected in Dr. Beddoe's well-known paper "On the Stature and Bulk of Man in the British Isles,"[8] a paper to which every student refers with unfailing confidence, and which will probably remain our standard authority until the labours of our own Anthropometric Committee are sufficiently matured for publication. Dr. Beddoe, summing up his observations on the physical characters of the Welsh as a whole, defines them as of "short stature, with good weight, and a tendency to darkness of eyes, hair, and skin." With regard to this tendency to darkness, it is well to look more searchingly at the district in which we are

[1] "Proximi Gallis et similes sunt."—*Agricola*, c. xi.
[2] "Mon. Hist. Brit.," Excerpta, p. lvi.
[3] "Plerosque Belgas esse ortos ab Germanis."—*De Bello Gall.*, lib. ii. c. 4.
[4] Lib. iv. c. i.
[5] "Quand César dit: *Hi omnes lingua, institutis, legibus, inter se differunt*, il faut traduire ici le mot *lingua* par *dialecte*."—"Les Derniers Bretons." Par Emile Souvestre, vol. i. p. 141.
[6] Lib. iv. c. 5.
[7] "Researches into the Physical History of Mankind." By J. C. Prichard, M.D., F.R.S., vol. iii. p. 189.
[8] *Mem.* Anthrop. Soc. Lond., vol. iii., 1870, p. 384.

assembled. Dr. Beddoe, in another paper,[1] indicated the tendency by a numerical expression which he termed the *index of nigrescence*. "In the coast-districts and lowlands of Monmouthshire and Glamorgan, the ancient seats of Saxon, Norman, and Flemish colonisation, I find," says this observer, "the indices of hair and eyes so low as 33·5 and 63; while in the interior, excluding the children of English and Irish immigrants, the figures rise to 57·3 and 109·5—this last ratio indicating a prevalence of dark eyes surpassing what I have met with in any other part of Britain" (p. 43).

Many years ago Mr. Matthew Moggridge, whose scientific work is well known in this district, furnished the authors of the "Crania Britannica" with notes of the physical characteristics of the Welsh of Glamorganshire. He defined the people as having "eyes (long) bright, of dark or hazel colour, hair generally black, or a very dark brown, lank, generally late in turning grey."[2]

There can be no question then as to the prevalence of melanism in this district. Nor does it seem possible to account for this tendency, as some anthropologists have suggested, by the influence of the surrounding media. Even those who believe most firmly in the potency of the environment will hardly be inclined to accept the opinion seriously entertained some years ago by the Rev. T. Price, that the black eyes of Glamorganshire are due to the prevalence of coal fires.[3] Long before coal came into use there was the same tendency to nigrescence among the Welsh. This may be seen, as Dr. Nicholas has pointed out, in the bardic names preserved in ancient Welsh records, where the cognomen of *du*, or "black," very frequently occurs. Thus, in the "Myvyrian Archaiology of Wales," between A.D. 1280 and 1330, there are registered four "blacks" to one "red" and one "grey," namely Gwilym *Ddu*, Llywelyn *Ddu*, Goronwy *Ddu*, and Dafydd *Ddu*.[4]

The origin of this dark element in the Welsh is to be explained, as every one will have anticipated, by reference to the famous passage in Tacitus, which has been worn threadbare by ethnologists. Tacitus tells us that the ancient British tribe of Silures—a tribe inhabiting what is now Glamorganshire, Monmouthshire, Herefordshire, and parts at least of Brecknockshire and Radnor—had a swarthy complexion, mostly with curly hair, and that from their situation opposite to Spain there was reason to believe that the Iberians had passed over the sea and gained possession of the country.[5] It will be observed that although Tacitus speaks of their dark complexion, he does not definitely state that the hair was dark; but this omission has, curiously enough, been supplied by Jornandes, a Goth who, in the sixth century, wrote a work which professes to be an extract from the lost history of Cassiodorus, wherein the very words of Tacitus are reproduced with the necessary addition.[6] With these passages before us, can we reasonably doubt that the swart blood in the Welsh of the present day is a direct legacy from their Silurian ancestors?

Setting what Tacitus here says about the Silures against what he says in the next sentence about the Britons nearest to Gaul (p. 5), it is clear that we must recognise a duality of type in the population of Southern Britain in his day. This fact has been clearly pointed out by Prof. Huxley as one of the few "fixed points in British ethnology."[7] At the dawn of history in this country, eighteen centuries ago, the population was not homogeneous, but contained representatives both of Prof. Huxley's *Melanochroi* and of his *Xanthochroi*. If we have any regard whatever for the persistence of anthropological types, we should hesitate to refer both of these to one and the same elementary stock. We are led then to ask, Which of these two types, if either, is to be regarded as Keltic?

It is because both of these types, in turn, have been called

[1] "On the Testimony of Local Phenomena in the West of England to the Permanence of Anthropological Types."—*Ibid.* vol. ii. 1866, p. 37.
[2] "Cran. Brit.," vol. i. p. 53.
[3] "Essay on the Physiognomy and Physiology of the Present Inhabitants of Britain," 1829.
[4] "The Pedigree of the English People," fifth edition, 1878, p. 467.
[5] "Silurum colorati vultus et torti plerumque crines, et posita contra Hispania, Ibeoros veteres trajecisse, easque sedes occupasse, fidem faciunt." —*Aricola*, c. xi.
[6] "Sylorum [= Silurum] colorati vultus, torto plerique crine, *et nigro* nascuntur."—"De Rebus Geticis," c. ii.; quoted in "Mon. Hist. Brit.," Excerpta, p. lxxxiii. It is conjectured that the classical word *Silures* is derived from the British name *Essyllwyr*, the people of *Essyllwg*. See Nicholas's "History of Glamorganshire," 1874, p. 1. It is difficult to determine how far and in what respects the Silures resembled, or differed from, the other inland tribes. Of the Caledonians and of the Belgæ we know something, but of the other inhabitants we are quite ignorant.
[7] "Critiques and Addresses," p. 166.

Keltic that so much confusion has been imported into ethnological nomenclature; hence the common-sense conclusion seems to be that neither type can strictly be termed Keltic, and that such a term had better be used only in linguistic anthropology.[1] The Kelt is merely a person who speaks a Keltic language, quite regardless of his race, though it necessarily follows that all persons who speak these languages, if not actually of one blood, must have been, at some period of their history, in close social contact. In this sense, all the inhabitants of Britain, at the period of the Roman invasion, notwithstanding the distinction between Xanthochroi and Melanochroi, were probably to be styled Kelts. There can be little doubt that the xanthous Britons always spoke a Keltic tongue; but it is not so easy to decide what was the original speech of their melanochroic neighbours.

The existence of two types of population, dark and fair, side by side, is a phenomenon which was repeated in ancient Gaul. As the Silures were to Britain so were the Aquitani to Gaul—they were the dark Iberian element. Strabo states that while the natives of Keltic and Belgic Gaul resembled each other, the Aquitanians differed in their physical characters from both of these peoples, and resembled the Iberians. But Tacitus has left on record the opinion that the Silures also resembled the Iberians; hence the conclusion that the Silures and the Aquitanians were more or less alike. Now it is generally believed that the relics of the old Aquitanian population are still to be found lingering in the neighbourhood of the Pyrenees, being represented at the present day by the Basques. A popular notion has thus got abroad that the ancient Silures must have been remotely affined to the Basque populations of France and Spain. Nevertheless the modern Basques are so mixed a race that, although retaining their ancient language, their physical characters have been so modified that we can hardly expect to find in them the features of the old Silurians. Thus, according to the Rev. Wentworth Webster, the average colour of the Basque hair at the present day is not darker than chestnut.[2]

Neither does language render us any aid towards solving the Basque problem. If the Silures were in this country prior to the advent of the Cymry, and if they were cognate with the Basques, it seems only reasonable to suppose that some spoor of their Iberian speech, however scant, might still be lingering amongst us. Yet philologists have sought in vain for the traces of any Euskarian element in the Cymraeg. Prince Louis Lucien Bonaparte, perhaps the only philologist in this country who has a right to speak with authority on such a subject, has obligingly informed me that he knows of no connection whatever between the two languages. Still it must be remembered that the Iberian affinity of the Silures, suggested by the remark of Tacitus, does not necessarily mean Basque affinity. Some philologists have even denied that the Basques are Iberians.[3] All that we seek at present to establish is this—that the dark Britons, represented by the tribe of Silures, although they came to be a Keltic-speaking people, were distinct in race from the fair Britons, and therefore in all likelihood were originally distinct in speech. Nor should it be forgotten that relics of a pre-Keltic non-Aryan people have been detected in a few place-names in Wales. Thus, Prof. Rhys is inclined to refer to this category such names as Menapia, Mona, and Mynwy[4]—the last-named being a place (Monmouth) within the territory of the old Silures, where we are now assembled. We may also look for light upon this subject from a paper which will be laid before the department by Mr. Hyde Clarke. On the whole it seems to me safer to follow Prof. Rolleston in speaking of the dark pre-Keltic element as *Silurian* rather than as Basque or as Iberian.[5] ("British Barrows," p. 630.)

There is, however, quite another quarter to which the anthropologist who is engaged in this investigation may turn with fair promise of reward. I need scarcely remind any one in this department of the singularly suggestive paper which was written more than fifteen years ago by the late Dr. Thurnam, "On the

[1] An excellent argument against the employment of national names by anthropologists will be found in a paper by Mr. A. L. Lewis "On the Evils arising from the Use of Historical National Names as Scientific Terms."—*Journ.* Anthrop. Inst., vol. viii., 1879, p. 825.

[2] "The Basque and the Kelt," *Journ.* Anthrop. Inst., vol. v., 1876 p. 5.

[3] "La Langue Iberienne et la Langue Basque." Par M. van Eys. *Revue de Linguistique*, July, 1874.

[4] "Lectures on Welsh Philology," second edition, p. 181.

[5] W. von Humboldt in his famous essay, "Prüfung der Untersuchungen über die Urbewohner Hispaniens vermittelst der Vaskischen Sprache," does not admit, on philological evidence, any extension of the Iberians to this country. See c. 44: "Ueber den Aufenthalt iberischer Völkerschaften ausserhalb Iberien in den von Celten bewohnten Ländern."

Two Principal Forms of Ancient British and Gaulish Skulls."[1] The long-continued researches of this eminent archæological anatomist led him to the conclusion that the oldest sepulchres of this country—the chambered and other long barrows which he explored in Wilts and Gloucestershire—invariably contained the remains of a dolichocephalic people, who were of short stature, and apparently were unacquainted with the use of metals. The absence of metal would alone raise a suspicion that these elongated tumuli were older than the round, conoidal, or bell-shaped barrows, which contain objects of bronze, if not of iron, with or without weapons of stone, and commonly associated with the remains of a taller brachycephalic people.

Even before Dr. Thurnam forcibly pointed attention to this distinction, it had been independently observed by so experienced a barrow-opener as the late Mr. Bateman,[2] whose researches were conducted in quite another part of the country—the district of the ancient Cornavii. Moreover, Prof. Daniel Wilson's studies in Scotland had led him to conclude that the earliest population of Britain were dolichocephalic, and possessed, in fact, a form of skull which, from its boat-like shape, he termed *kumbecephalic.*[3] Nor should it be forgotten that as far back as 1844 the late Sir W. R. Wilde expressed his belief that in Ireland the most ancient type of skull is a long skull, which he held to belong to a dark-complexioned people, probably aboriginal, who were succeeded by a fair, round-headed race."[4]

But while this succession of races was recognised by several observers, it remained for Dr. Thurnam to formulate the relation between the shape of the skull and that of the barrow in a neat aphorism which has become a standing dictum in anthropology—"Long barrows, long skulls; round barrows, round skulls; dolichotaphic barrows, dolichocephalic crania; brachytaphic barrows, brachycephalic crania." No doubt exceptional cases may occur in which round skulls have been found in long barrows, but these have generally been explained as being due to secondary interments. On the other hand, the occasional presence of long skulls in round barrows presents no difficulty, since no one supposes that the early dolichocephali were exterminated by the brachycephali, and it is therefore probable that during the bronze-using period, when round tumuli were in general use, the two peoples may have dwelt side by side, the older race being, perhaps, in a state of subjugation.

It is not pretended that Thurnam's apophthegm has more than a local application. "This axiom," its author admitted, "is evidently not applicable, unless with considerable limitations, to France." Although it is here called an "axiom," it is by no means a self-evident proposition, the relation between the shape of the skull and the shape of the burial-mound being purely arbitrary. The proposition which connects the two is simply the expression of the results of accumulated observations, and it is of course open to doubt whether the number of observations was sufficiently great to warrant the generalisation. But the only test of the validity of any induction lies in its verification when applied to fresh instances, and it is remarkable that when long barrows and chambered tumuli have since been opened in this country the evidence has tended in the main to confirm Dr. Thurnam's proposition.

It is commonly believed that the brachycephali of the round barrows came in contact with the dolichocephali as an invading, and ultimately as a conquering, race. Not only were they armed with superior weapons—superior in so far as a metal axe is a better weapon than a stone axe—but they were a taller and more powerful people. Thurnam's measurements of femora led to the conclusion that the average height of the brachycephali was 5 feet 8·4 inches, while that of the long-headed men was only 5 feet 5·4 inches.[5] Not only were they taller, but they were probably a fiercer and more warlike race. In the skulls from the round barrows the superciliary ridges are more prominent, the nasals diverge at a more abrupt angle, the cheek-bones are high, and the lower jaw projects, giving the face an aspect of ferocity, which contrasts unfavourably with the mild features of the earlier stone-using people.

On the whole, then, the researches of archæological anatomists tend to prove that this country was tenanted in ante-historic or pre-Roman times by two peoples who were ethnically distinct from each other. It is difficult to resist the temptation of apply-

[1] *Memoirs* of the Anthrop. Soc. Lond., vol. i. 1865, p. 120; vol. iii. 1870, p. 41.

[2] "Ten Years' Diggings," 1861, p. 146.

[3] "Prehistoric Annals of Scotland," 1851.

[4] "On the Ethnology of the Ancient Irish."

[5] *Mem.* Anthrop. Soc. Lond., vol. iii. 1870, p. 73.

ing this to the ethnogeny of Wales. Does it not seem probable that the early short race of long-skulled, mild-featured, stone-using people may have been the ancestors of the swarthy Silurians of Tacitus; while the later tall race of round-skulled, rugged-featured, bronze-using men may have represented the broad-headed, Keltic-speaking folk of history? At any rate, the evidence of craniology does not run counter to this hypothesis. For Dr. Beddoe's observations on head-forms in the west of England have shown that "heads which are ordinarily called brachycephalic belonged for the most part to individuals with light hair," while the short dark-haired people whom he examined were markedly dolichocephalic.[1] At the same time it must be admitted that his observations lend "no support to the view that the Keltic skull has been, or would be, narrowed by an admixture of the Iberian type." It should not, however, be forgotten that the same observer, in referring to a collection of crania from the Basque country preserved in Paris, says: "The form of M. Broca's Basque crania was very much that of some modern Silurian heads."[2]

According to the view advocated by Thurnam we have a right to anticipate that the oldest skulls found in this country would be of dolichocephalous type; and such I believe to be actually the case. Dr. Barnard Davis, it is true, has stated in the "Crania Britannica that the ancient British skull must be referred to the brachycephalic type; and such an induction was perfectly legitimate so long as the craniologist dealt only with skulls from the round barrows or from similar interments. But the long-barrow skulls examined by Prof. Rolleston,[3] and the Cissbury skulls recently studied by the same anatomist,[4] are decidedly dolichocephalic, as also are all the early prehistoric skulls which have been found of late years in France. While referring to craniology in this country, I may perhaps be allowed to remark that the eminent Italian anthropologist, Dr. Paolo Mantegazza, in a suggestive paper which has just appeared in his valuable journal, the *Archivio per l'Antropologia*, has referred to the Englishman's contempt for craniological work—work but little worthy of the practical spirit of the Anglo-Saxon race.[5] No doubt it is desirable to increase the number of our observations, but still the good-humoured remark about despising craniology can hardly be applied to a country which numbers among its living men of science such eminent craniologists as Prof. Busk, Prof. Cleland, Dr. Barnard Davis, and Professors Flower, Huxley, and Rolleston.

It may naturally be asked whether the researches of archæologists in Wales lend any support to Thurnam's hypothesis. Nothing, I conceive, would be easier than to show that very material support has come from this quarter; but I have abstained, of set purpose, from introducing into this address any remarks on the prehistoric archæology of Wales. For I have not forgotten that we are to have the privilege of hearing an evening lecture on "Primæval Man" by so distinguished an archæologist and naturalist as Prof. Boyd Dawkins. No one has done more in this country to forward Thurnam's views, whether by actual exploration or by writing, than Prof. Boyd Dawkins; and if I have not referred to his work, especially to his discoveries in Denbighshire, it has been simply because I was anxious to avoid trespassing on any subject which he is likely to bring forward.[6]

Setting aside, then, any archæological evidence derived from the bone-caves, barrows, or other sepulchres in Wales, we may finally look at the outcome of our inquiry into Welsh ethnogeny. If we admit, as it seems to me we are bound to admit, the existence of two distinct ethnical elements in the Welsh population, one of which is short, dark, and dolichocephalic—call it Silurian, Atlantean, Iberian, Basque, or what you will; and the other of which is tall, fair, and brachycephalic, such as some term Cymric and others Ligurian; then it follows that by the crossing of these two races we may obtain not only individuals of intermediate character, but occasionally more complex combinations; for example, an individual may have the short stature and long head of the one race, associated with the lighter hair of the other; or again, the tall stature of one may be found in association with the melanism and dolichocephalism of the other race. It is therefore no objection to the views herein expressed if we can point to a living Welshman who happens to be at once tall and dark, or to another who is short and fair.

At the same time I am by no means disposed to admit that when we have recognised the union of the xanthous and melanic elements in Wales, with a predominance of the latter in the south, we have approached to anything like the exhausting limit of the subject. Still earlier races may have dwelt in the land, and have contributed something to the composition of the Welsh. In fact the anthropologist may say of a Welshman, as a character in "Cymbeline" says of Posthumus when doubtful about his pedigree—

"I cannot delve him to the root."

It is possible that the roots of the Welsh may reach far down into some hidden primitive stock, older mayhap than the Neolithic ancestors of the Silurians; but of such pristine people we have no direct evidence. So far however as positive investigation has gone, we may safely conclude that the Welsh are the representatives, in large proportion, of a very ancient race or races; and that they are a composite people who may perhaps be best defined as *Siluro-Cymric*.

Many other questions relating to Welsh ethnology press for consideration—such as the hypothesis that the Kymro was preceded, in parts at least of Wales, by the Gael; but such questions must be dismissed from present discussion, for I fear that my remarks have already overrun the limits of a departmental address. Let us hope however that much light may be thrown upon a variety of questions bearing upon local anthropology in the course of the discussions which will arise in this department during the present session of the Association.

[1] *Mem.* Anthrop. Soc. Lond., vol. ii. 1866, p. 350. [2] *Ibid.*, p. 356.

[3] "On the People of the Long Barrow Period," *Journ.* Anthrop. Inst., vol. v., 1876, p. 120.

[4] *Ibid.*, vol. vi. 1877, p. 20; vol. viii., 1879, p. 377.

[5] The whole passage so amusingly refers to the national idiosyncracies of craniologists, that it is well worth reproduction. "In Francia, Broca, il pontefice massimo dell' *ipercraniologia* moderna, col suo ardore eternamente giovanile, non studia più i crani, ma i cervelli; in Germania si prendono ancora misure sui teschi, ma con *rationabile obsequio*, quasi si dovesse adempiere ad un dovere noioso; in Inghilterra si continua a sprezzare la craniologia, come cosa poco degna dello spirito pratico della razza anglosassone; e in Italia, paese più scettico di tutti, perchè più antico e più stanco di tutti, si continua a misurare, pur sorridendo dell' improba e pur inutile fatica." —"La Riforma Craniologica;" *Archivio*, vol. x., 1880, p. 117.

[6] For Prof. Boyd Dawkins' contributions to the subject see his interesting works on "Cave-hunting," 1874, and on "Early Man in Britain," 1880.

PALÆONTOLOGICAL AND EMBRYOLGICAL DEVELOPMENT[1]

SINCE the publication of the "Poissons Fossiles" by Agassiz and of the "Embryologie des Salmonidées" by Vogt, the similarity, traced by the former between certain stages in the growth of young fishes and the fossil representatives of extinct members of the group, has also been observed in nearly every class of the animal kingdom, and the fact has become a most convenient axiom in the study of palæontological and embryological development. This parallelism, which has been on the one side a strong argument in favour of design in the plan of creation, is now, with slight emendations, doing duty on the other as a newly-discovered article of faith in the new biology.

But while in a general way we accept the truth of the proposition that there is a remarkable parallelism between the embryonic development of a group and its palæontological history, yet no one has attempted to demonstrate this, or rather to show how far the parallelism extends. We have up to the present time been satisfied with tracing the general coincidence, or with striking individual cases.

The resemblance between the pupa stage of some Insects and of adult Crustacea, the earlier existence of the latter, and the subsequent appearance of the former in palæontological history, furnished one of the first and most natural illustrations of this parallelism; while theoretically the necessary development of the higher tracheate insects from their early branchiate aquatic ancestors seemed to form an additional link in the chain, and point to the Worms, the representatives of the larval condition of Insects, as a still earlier embryonic stage of the Articulates.

Indeed there is not a single group of the animal kingdom in which embryology has not played a most important part in demonstrating affinities little suspected before. The development of our frogs, our salamanders, has given us the key to much that was unexplained in the history of Reptiles and Batrachians. The little that has been done in the embryology of Birds has revolutionised our ideas of a class which at the beginning of the century seemed to be the most naturally circumscribed of all. Embryology and palæontology combined have led to the recognition of a a natural classification uniting Birds and Reptiles on

[1] Address by Prof. Alexander Agassiz, vice-president, Section B, at the Boston Meeting of the American Association, August, 1880.

the one side, and Batrachians and Fishes on the other. It is to embryology that we owe the explanation of the affinities of the Old Fishes in which Agassiz first recognised the similarity to the embryo of Fishes now living, and by its aid we may hope to understand the relationship of the oldest representatives of the class. It has given us the only explanation of the early appearance of the Cartilaginous Fishes, and of the probable formation of the earliest vertebrate limb from the lateral embryonic fold, still to be traced in the young of the Osseous Fishes of to-day.

Embryology has helped us to understand the changes aquatic animals must gradually undergo in order to become capable of living upon dry land. It has given us pictures of swimming-bladders existing as rudimentary lungs in Fishes with a branchial system; in Batrachians it has shown us the persistence of a branchial system side by side with a veritable lung. We find among the earliest terrestrial vertebrates types having manifest affinities with the Fishes on one side and Batrachians on the other, and we call these types Reptiles; but we should nevertheless do so with a reservation, looking to embryology for the true meaning of these half-fledged Reptiles, which lived at the period of transition between an aquatic and a terrestrial life, and must therefore always retain an unusual importance in the study of the development of animal life.

When we come to the embryology of the marine Invertebrates the history of the development of the barnacles is too familiar to be dwelt upon, and I need only allude to the well-known transformations of the Echinoderms, of the Acalephs, Polyps, in fact of every single class of Invertebrates, and perhaps in none more than in the Brachiopods, to show how far-reaching has been the influence of embryology in guiding us to a correct reading of the relations between the fossils of successive formations. There is scarcely an embryological monograph now published dealing with any of the later stages of growth which does not speak of their resemblance to some type of the group long ago extinct. It has therefore been most natural to combine with the attempts constantly made to establish the genetic sequence between the genera of successive formations an effort to establish also a correspondence between their palæontological sequence and that of the embryonic stages of development of the same, thus extending the mere similarity first observed between certain stages to a far broader generalisation.

It would carry me too far to sketch out, except in a most general way, even for a single class, the agreement known to exist in certain groups between their embryonic development and their palæontological history. It is hinted at in the succession of animal life of any period we may take up, and perhaps cannot be better expressed than by comparing the fauna of any period as a whole with that of following epochs;—a zoological system of the Jura, for instance, compared with one made up for the Cretaceous; next, one for the Tertiary, compared with the fauna of the present day. In no case could we find any class of the animal kingdom bearing the same definitions or characterised in the same manner. But apply to this comparison the data obtained from the embryological development of our present fauna, and what a flood of light is thrown upon the meaning of the succession of these apparently disconnected animal kingdoms, belonging to different geological periods, especially in connection with the study of the few ancient types which have survived to the present day from the earliest times in the history of our earth!

Although there is hardly a class of the animal kingdom in which some most interesting parallelism could not be drawn, and while the material for an examination of this parallelism is partially available for the Fishes, Molluscs, Crustacea, Corals, and Crinoids, yet for the illustration and critical examination of this parallelism I have been led to choose to-day a very limited group, that of Sea-urchins, both on account of the nature of the material and of my own familiarity with their development and with the living and extinct species of Echini. The number of living species is not very great,—less than three hundred,—and the number of fossil species thus far known is not, according to Zittel, more than about two thousand. It is therefore possible for a specialist to know of his own knowledge the greater part of the species of the group. It has been my good fortune to examine all but a few of the species now known to exist, and the collections to which I have had access contain representatives of the majority of the fossil species. Sea-urchins are found in the oldest fossiliferous rocks; they have continued to exist without interruption in all the strata up to the present time. While it is true that our knowledge of the Sea-urchins occurring before the Jurassic period is not very satisfactory, it is yet complete enough for the purposes of the present essay, as it will enable me, starting from the Jurassic period, to call your attention to the palæontological history of the group, and to compare the succession of its members with the embryological development of the types now living in our seas. Ample material for making this comparison is fortunately at hand; it is material of a peculiar kind, not easily obtained, and which thus far has not greatly attracted the attention of zoologists.

Interesting and important as are the earliest stages of embryonic development in the different classes of the animal kingdom, as bearing upon the history of the first appearance of any organ and its subsequent modifications, they throw but little light on the subject before us. What we need for our comparisons are the various stages of growth through which the young Sea-urchins of different families pass from the time they have practically become Sea-urchins until they have attained the stage which we now dignify with the name of species. Few embryologists have carried their investigations into the more extended field of the changes the embryo undergoes when it begins to be recognised as belonging to a special class, and when the knowledge of the specialist is absolutely needed to trace the bearing of the changes undergone, and to understand their full meaning. Fortunately the growth of the young Echini has been traced in a sufficient number of families to enable me to draw the parallelism between these various stages of growth and the palæontological stages in a very different manner from what is possible in other groups of the animal kingdom, where we are overwhelmed with the number of species, as in the Insects or Mollusks, or where the palæontological or the embryological terms of comparison are wanting or very imperfect.

Beginning with the palæontological history of the regular sea-urchins at the time of the Trias, when they constituted an unimportant group as compared with the Crinoids, we find the Echini of that time limited to representatives of two families. One of these, the genus Cidaris, has continued to exist, with slight modifications, up to the present time, and not less than one-tenth of all the known species of fossil Echini belong to this important genus, which in our tropical seas is still a prominent one. It is interesting here to note that in the Cidaridæ the modifications of the test are not striking, and the fossil genera appearing in the successive formations are distinguished by characters which often leave us in doubt as to the genus to which many species should be referred. In the genus Rhabdocidaris, which appears in the lower Jura, and which is mainly characterised by the extraordinary development of the radioles, we find the extreme of the variations of the spines in this family. From that time to the present day the most striking differences have existed in the shape of the spines, not only of closely allied genera, but even in specimens of the same species; differences which in some of the species of to-day are as great as in older geological periods. The oldest Cidaridæ are remarkable for their narrow poriferous zones. It is only in the Jura that they widen somewhat; subsequently the pores become conjugated, and only later, during the Cretaceous period, do we find the first traces of any ornamentation of the test (Temnocidaris) so marked at the present day in the genus Goniocidaris. As far, then, as the Cidaridæ are concerned, the modifications which take place from their earliest appearance are restricted to slight changes in the poriferous zone and in the ornamentation of the test, accompanied with great variability in the shape of the primary radioles. We must except from this statement the genera Diplocidaris and Tetracidaris, to which I shall refer again. The representatives of the other Triassic family become extinct in the lower tertiaries. The oldest genus, Hemicidaris, undoubtedly represents the earliest deviations from the true Cidaris type; modifications which affect not only the poriferous zone, but the test, the actinal and the abactinal systems, while from the extent of these minor changes we can trace out the gradual development of some of the characteristics in families of the regular Echini now living. The genus Hemicidaris may be considered as a Cidaris in which the poriferous zone is narrow and undulating, in which the granules of the ambulacral system have become minute tubercles in the upper portion of the zone and small primary tubercles in its actinal region, in which many of the inter-ambulacral granules become small secondaries, in which the plates of the actinal system have become reduced in number, and the apical system has become a narrow ring, and finally, in which the primary radioles no longer assume the fantastic shapes so common among the Cidaridæ.

We can trace in this genus the origin of the modifications of the poriferous zone, leading us, on the one side, through genera with merely undulating lines of pores to more or less distinct confluent arcs of pores, formed round the primary ambulacral tubercles, and, on the other, to the formation of open arcs of three or more pairs of pores. The first type culminates at the present day with the Arbaciadæ, the other with the Diadematidæ, Triplechinidæ, and Echinometradæ. This specialisation very early takes place, for already in the lower Jura Stomechinus has assumed the principal characteristics of the Triplechinidæ of to-day.

Although in Hemicidaris the number of the coronal plates has increased as compared with the Cidaridæ, and while we find that in many genera, even of those of the present day, the number of the coronal plates is still comparatively small, yet, as a general rule, the more recent formations contain genera in which the increase in number of the interambulacral plates is accompanied by a corresponding decrease in the number of plates of the interambulacral area so characteristic thus far of the Cidaridæ and Hemicidaridæ, a change also affecting the size of the primary ambulacral tubercles. This increase in the number of the coronal plates is likewise accompanied by the development of irregular secondary and miliary tubercles, and the disappearance in this group of the granular tuberculation, so important a character in the Cidaridæ. With the increase in the number of the interambulacral coronal plates, the Pseudodiadematidæ still retain prominent primary tubercules, recalling the earlier Hemicidaridæ and Cidaridæ, and, as in the Cidaridæ proper, the test is frequently ornamented by deep pits or by ridges formed by the junction of adjoining tubercles. The genital ring becomes narrower, and the tendency to the specialisation of one of its plates, the madreporite, more and more marked.

With the appearance of Stomechinus, the Echinidæ proper already assume in the Jura the open arcs of pores, the large number of coronal interambulacral plates, the specialisation of the secondary tubercles, and the large number of primary tubercles in each plate. With the appearance of Sphærechinus in the early Tertiary come in all the elements for the greater multiplication of the pairs of pores in the arcs of the poriferous zones, while the gigantic primary spines of some of the genera (Heterocentrotus), and the small number of primary tubercles are structural features which had completely disappeared in the group preceding the Echinometradæ, to which they appear most closely allied.

Going back again to the Hemicidaridæ, it requires but slight changes to pass from them to Acrosalenia and to the Saleniæ proper; the latter have continued to the present day, and have, like the Cidaridæ, retained almost unchanged the characters of the genera which preceded them, combined however with a few Cidaridian and Echinid features which date back to the Triassic period. We can thus trace the modifications which have taken place in the poriferous zone, the apical and actinal systems, the coronal plates, the ambulacral and interambulacral tubercles, as well as in the radioles, and in the most direct manner possible indicate the origin of the peculiar combination of structural features which we find at any geological horizon. On taking in succession the modifications undergone by the different parts of the test, we can trace each one singly, without the endless complication of combinations which any attempt to trace the whole of any special generic combination would imply.

Leaving out of the question for the moment the Palæchinidæ, we find no difficulty in tracing the history of the characters of the genera of the regular Echini which have existed from the time of the Trias and are now living, provided we take up each character independently. Nothing can be more direct than the gradual modification of the simple, barely undulating poriferous zone, made up of numerous ambulacral plates covered by granules, such as we find it among the Cidaridæ of the Trias, first into the slightly undulating poriferous zone of the Hemicidaridæ, next into the indistinct arcs of pores of the Pseudodiadematidæ, then into the arcs with a limited number of pores of the Triplechinidæ, and finally to the polyporous arcs of the Echinometradæ. What can be more direct than the gradual modification to be traced in the development of the primary ambulacral tubercles, such as are characteristic of the Echinidæ of the present day, from their first appearance at the oral extremity of the ambulacral system of the Hemicidaridæ, and the increase in the number of primary interambulacral tubercles, accompanied by the growth of secondaries and miliaries, which we can trace in Hemicidaris, Acrosalenia, and Stomechinus—the increase in number of the primary and secondary tubercles being accompanied by a reduction in the size of the radioles and a greater uniformity in their size and shape?

But while these modifications take place the original structural feature may be retained in an allied group. Thus the Cidaridæ retain unchanged from the earliest time to the present day the few primary tubercles, the secondary granules, the simple poriferous zone, the imbricating actinal system, and the few coronal plates, with the large apical system and many-shaped radioles; while in the Salenidæ the primary interambulacral tubercles, the secondary granules, the radioles, the genital ring, are recognised features of the Cidaridæ, associated, however, with an Echinid actinal and anal system, Hemicidarid primary ambulacral tubercles, and an Echinid poriferous zone. In the same way in the Diadematidæ, the large primary interambulacral tubercles are Cidaridian features, while the structure of the ambulacral tubercles is Hemicidaridian. The existence of two kinds of spines is another Cidaridian feature, while the apical and actinal systems have become modified in the same direction as that of the Echinidæ. The more recent the genus the greater is the difficulty of tracing in a direct manner the origin of any one structural feature, owing to the difficulty of disassociating structural elements characteristic of genera which may be derived from totally different sources. This is particularly the case with genera having a great geological age. Many of them, especially among the Spatangoids, show affinities with genera following them in time, to be explained at present only on the supposition that when a structural feature has once made its appearance it may reappear subsequently apparently as a new creation, while in reality it is only its peculiar combination with structural features with which it had not before been associated (a new genus), which conceals in that instance the fact of its previous existence. A careful analysis, not only of the genera of the order, but sometimes of other orders which have preceded this combination in time, may often reveal the elements from which have been produced apparently unintelligible modifications.

There is, however, not one of the simple structural features in the few types of the Triassic and Liassic Echini from which we can so easily trace the origin of the structural features of all the subsequent Echinid genera, which is not also itself continued to the present day in some generic type of the present epoch, fully as well characterised as it was in the beginning. In fact, the very existence to-day of these early structural features seems to be as positive a proof of the unbroken systematic affinity between the Echini of our seas and those of the Trias as the uninterrupted existence of the genus Pygaster or Cidaris from the Trias down to the present epoch, or of the connection of many of the genera of the Chalk with those of our epoch (Salenia, Cyphosoma, Psammechinus, &c.).

Passing to the Clypeastridæ, we find there, as among the Desmosticha, that the earliest type, Pygaster, has existed from the Trias to the present time; and that, while we can readily reconstruct, on embryological grounds, the modifications the earliest Desmosticha-like Echini should undergo in order to assume the structural features of Pygaster, yet the early periods in which the precursors of the Echinoconidæ and Clypeastridæ are found have thus far not produced the genera in which these modifications actually take place. But, starting from Pygaster, we naturally pass to Holectypus, to Discoidea, to Conoclypus, on the one side, while on the other, from Holectypus to Echinocyamus, Sismondia, Fibularia, and Mortonia, we have the natural sequence of the characters of the existing Echinanthidæ, Laganidæ, and Scutellidæ, the greater number of which are characteristic of the present epoch. If we were to take in turn the changes undergone in the arrangement of the plates of the test, as we pass from Pygaster to Holectypus, to Echinocyamus, and the Echinanthidæ, we should have in the genera which follow each other in the palæontological record an unbroken series showing exactly what these modifications have been. In the same way the modifications of the abactinal and anal systems, and those of the poriferous zone, can equally well be followed to Echinocyamus, and thence to the Clypeastridæ; while a similar sequence in the modifications of these structural features can be followed from Mortonia to the Scutellidæ of the present period.

Passing finally to the Petalosticha, we find no difficulty in tracing theoretically the modifications which our early Echinoconidæ of the Lias should primarily undergo previous to the appearance of Galeropygus. The similarity of the early Cassiduloid and Echinoneoid types points to the same systematic

affinity, and perhaps even to a direct and not very distant relationship with the Palæchinidæ. For if we analyse the Echinothuriæ of the present day, we find in genera like Phormosoma many structural features, such as the shape of the test, the character of the spines, the structure of the apical system, that of the poriferous zone, indicative of possible modifications in the direction of Pygaster or of Galeropygus, which have as yet not been taken into account.

Adopting for the Petalosticha the same method of tracing the modifications of single structural features in their palæontological succession, we trace the comparatively little modified palæontological history of the Echinoneidæ of the present day from the Pyrina of the lower Jura. This, in its turn, has been preceded by Hyboclypus and Galeropygus, while the Echinolampadæ of the present day date back, with but trifling modifications, to the Echinobrissus of the Lias, itself preceded by Clypeus: and they have been subject only to slight generic changes since that time, Echinobrissus being still extant, while such closely-allied genera as Catopygus and Cassidulus of the earlier Cretaceous are still represented at the present day; the modifications taking place in the actinal system, in the ambulacral zones of the Echinoconidæ and of the Echinolampadæ showing the closest possible systematic affinity in these families. Starting again from Hyboclypus, with its elongate apical system, we naturally pass to Collyrites and the strange Dysasteridæ forms which in their turn are closely allied to the Holasteridæ. From Holaster on the one side and from Toxaster on the other, we find an unbroken sequence of structural characters uniting the successive genera of Holasteridæ, such as Cardiaster, Offaster, Stenonia, Ananchytes, and Asterostoma, with Paleopneustes, Homolampas, and the Pourtalesiæ of the present day, while from the genera of the Toxasteridæ we naturally pass to the cretaceous Hemiaster; in this genus and the subsequent Micraster we find all the elements necessary for the modifications which appear in the Spatanginæ from the time of the Chalk to the present day. These modifications result in genera in which we trace the development of the fascioles, of the actinal, anal, and abactinal plastrons, of the beak, the formation of the petaloid ambulacra, first flush with the test, and little by little changed into marsupial pouches, the growth of the anterior groove and the manifold modifications of the ambulacral system in Spatangus, Agassizia, and Echinocardium, often recalling in some of its features structural characters of families which have preceded this in time.

Apparently in striking contrast with the Echini of the secondary period and those which have succeeded them stand the Palæozoic Echini; but when we have examined the embryology of Echini, we shall be better prepared to understand their structure and the affinities of the Palæchinidæ with the Echini of the present day, and their immediate predecessors.

Taking up now the embryological development of the several families which will form the basis of our comparisons, beginning with the Cidaridæ, we find that in the earliest stages they very soon assume the characters of the adult, the changes being limited to the development of the abactinal system, the increase in number of the coronal plates, and the modifications of the proportionally gigantic primary radioles.

In the Diadematidæ the changes undergone by the young are limited to the gradual transformation of the embryonic spines to those which characterise the family, to the changes of the vertical row of pores in the ambulacral area into arcs of three or four pairs of pores, and to the specialisation of the actinal and abactinal systems.

In the Arbaciadæ the young stages are remarkable for the prominent sculpture of the test, for the flattened spines, for their simple poriferous zone, for their actinal system, and for their genital ring. The anal plates appear before the genital ring.

In the Echinometradæ the young thus far observed are characterised by the small number of their primary tubercles, the large size of the spines, the simple vertical row of pores, the closing of the anal ring by a single plate, and the turban-shaped outline of the test. Little by little the test loses with increasing age this Cidaris-like character; it reminds us, from the increase in the number of its plates, more of Hemicidaris; then, with their still greater increase, of the Pseudodiadematidæ; and, finally, of the Echinometradæ proper. The spines, following *pari passu* the changes of the test, lose little by little their fantastic embryonic, or rather Cidaris-like appearance, and become more solid and shorter, till they finally assume the delicately fluted structure characteristic of the Echinometradæ. The vertical poriferous zone is first changed into a series of connected vertical arcs, which become disjointed, and form, with increasing age, the independent arcs of pores, of three or more pairs of pores, of the Echinometradæ.

In the Echinidæ proper we find in the young stages the same unbroken vertical line of pores, which gradually becomes changed to the characteristic generic types. We find, as in the Echinometradæ, an anal system closed with a single plate, and an abactinal system separating in somewhat more advanced stages from the coronal plates of the test. This is as yet made up of a comparatively small number of plates, carrying but few large primary tubercles, with fantastically shaped spines entirely out of proportion to the test, but which, little by little, with the increase of the number of coronal plates, the addition of primary tubercles, and their proportional decrease in size, assume more and more the structure of the genus to which the young belongs. The original anal plate is gradually lost sight of from the increase in number of the plates covering the anal system, and it is only among the Temnopleuridæ that this anal plate remains more or less prominent in the adult. In the Salenidæ, of which we know as yet nothing of the development, this embryonic plate remains permanently a prominent structural feature of the apical system.[1]

Among the Clypeastroids the changes of form they undergo during growth are most instructive. We have in the young Fibularinæ an ovoid test, a small number of coronal plates surmounted by few and large primary tubercles, supporting proportionally equally large primary radioles, simple rectilinear poriferous zones, no petaloid ambulacra—in fact, scarcely one of the features we are accustomed to associate with the Clypeastroids is as yet prominently developed. But rapidly, with increasing size, the number of primary tubercles increases, the spines lose their disproportionate size, the pores of the abactinal region become crowded and elongate, and a rudimentary petal is formed. The test becomes more flattened, the coronal plates increase in number, and it would be impossible to recognise in the young Echinocyamus, for instance, the adult of the Cidaris-like or Echinometra-like stages of the Sea-urchin, had we not traced them step by step. Most interesting, also, is it to follow the migrations of the anal system, which, to a certain extent, may be said to retain the embryonic features of the early stages of all Echinoderm embryos, in being placed in more or less close proximity to the actinostome. What has taken place in the growth of the young Echinocyamus is practically repeated for all the families of Clypeastroids; a young Echinarachnius, or Mellita, or Encope, or a Clypeaster proper, resembles at first more an Echinometra than a Clypeastroid; they all have simple poriferous zones and spines and tubercles out of all proportion to the size of the test.[2]

When we come to the development of the Spatangoids, we find their younger stages also differing greatly from the adult. Among the Nucleolidæ, for instance, the young stages have as yet no petals, but only simple rectilinear poriferous zones. They are elliptical with a high test, with a single large primary tubercle for each plate, and a simple elliptical actinostome, without any trace of the typical bourrelets and phyllodes so characteristic of this family. Very early, however, this condition of things is changed, the test soon becomes more flattened, the petals begin to form as they do in the Clypeastroids, and we can soon trace the rudiments of the peculiar bourrelets characteristic of the family, accompanied by a rapid increase in the number of tubercles and in that of the coronal plates.

Among the Spatangidæ some are remarkable in their adult condition for their labiate actinostome, for the great development of the petals, for the presence of fascioles surrounding certain definite areas, for the small size of the tubercles, the general uniformity in the spines of the test, and the specialisation of their anterior and posterior regions. On examining the young stages of this group of Spatangoids, not one of these structural features is as yet developed. The actinostome is simple, the poriferous zone has the same simple structure from the actinostome to the apex, the primary tubercles are large, few in number, surrounded by spines which would more readily pass as the spines of Cidaridæ than of Spatangoids. The fascioles are either very indistinctly indicated, or else the special lines have

[1] The young of the following genera have served as a basis for the preceding analysis of the embryonic stages of the Desmosticha: Cidaris, Dorocidaris, Goniocidaris, Arbacia, Porocidaris, Strongylocentrotus, Echinometra, Echinus, Toxopneustes, Hipponoë, Temnopleurus, Temnechinus, and Trigonocidaris.

[2] Among the Clypeastroids I have examined the young of Echinocyamus, Fibularia, Mellita, Laganum, Echinarachnius, Encope, Clypeaster, and Echinanthus.

not as yet made their appearance; the ambulacral suckers of the anterior zone are as large and prominent as those of the young stages of any of the regular Echini. It is only little by little, with advancing age, that we begin to see signs of the specialisation of the anterior and posterior parts of the test, that we find the characteristic anal or lateral fascioles making their appearance, only with increasing size that the spines lose their Cidaris-like appearance, that the petals begin to be formed, and that the simple actinostome develops a prominent posterior life. In the genus Hemiaster, the young stages are specially interesting, as long before the appearance of the petals, while the poriferous zone is still simple, the total separation of the bivium and of the trivium of the ambulacral system, so characteristic of the earliest Spatangoids (the Dysasterinæ), is very apparent.[1]

From this rapid sketch of the changes of growth in the principal families of the recent Echini we can now indicate the transformations of a more general character through which the groups as a whole pass.

In the first place, while still in the Pluteus all the young Echini are remarkable for the small number of coronal plates, for the absence of any separation between the actinal and abactinal systems and the test proper. They all further agree in the large size of the primary spines of the test, whether it be the young of a Cidaris, an Arbacia, an Echinus, a Clypeaster, or a Spatangoid. They all in their youngest stages have simple vertical ambulacral zones; beyond this, we find as changes characteristic of some of the Desmosticha, the specialisation of the actinal system from the coronal plates, the formation of an anal system, the rapid increase in the number of the coronal plates, with a corresponding increase in the number of the spines and a proportional reduction of their size, the formation of an abactinal ring, and the change of the simple vertical poriferous zone into one composed of independent arcs.

In the Spatangoids and Clypeastroids we find common to both groups the shifting of the anal system to its definite place, the modifications of the abactinal part of the simple ambulacral system in order to become petaloid, and the gradual change of the elliptical ovoid test of the young to the characteristic generic test, accompanied by the rapid increase in the number of the primary tubercles and spines. Finally limited to the Spatangoids are the changes they undergo in the transformation of the simple actinostome to a labiate one, the specialisation of the anterior and posterior parts of the test, and the definite formation of the fascioles.

Comparing this embryonic development with the palæontological one, we find a remarkable similarity in both, and in a general way there seems to be a parallelism in the appearance of the fossil genera and the successive stages of the development of the Echini as we have traced it.

We find that the earlier regular Echini all have more or less a Cidaris-like look—that is, they are Echini with few coronal plates, large primary tubercles, with radioles of a corresponding size; that it is only somewhat later that the Diademopsidæ make their appearance, which, in their turn, correspond within certain limits to the modifications we have traced in the growth of the young Diadematidæ and Arbaciadæ. The separation of the actinal system from the coronal plates has been effected. The poriferous zone has either become undulating, or forms somewhat indefinite open arcs; we find in all the genera of this group a larger number of coronal plates, more numerous primaries, the granules of the Cidaridæ replaced by secondaries and miliaries, and traces of a Hemicidaris-like stage in the size of the actinal ambulacral tubercles.

Comparing in the same way the palæontological development of the Echinidæ proper, we find that, on the whole, they agree well with the changes of growth we can still follow to-day in their representatives, and that, as we approach nearer the present epoch, the fossil genera more and more assume the structural features which we find developed last among the Echinidæ of the present day. Very much in the same manner as a young Echinus develops, they lose, little by little, first their Cidaridian affinities, which become more and more indefinite, next their Diadematidian affinities, if I may so call the young stages to which they are most closely allied, and finally, with the increase in the number of the coronal plates, the great numerical development of the primary tubercles and spines, and that of the secondaries and miliaries which we can trace in the fossil Echini of the Tertiaries, we pass insensibly into the generic types characteristic of the present day.

Although we know nothing of the embryology of the Salenidæ, yet, like the Cidaridæ, they have in a great measure remained a persistent type, the modifications of the group being all in the same direction as those noticed in the other Desmosticha; a greater number of coronal plates, the development of secondaries and miliaries combined with a specialisation of the actinal system not found in the Cidaridæ.

An examination of the succession of the Echinoconidæ shows but little modification from the earliest types; the changes, however, are similar to those undergone by the Clypeastroids and Petalosticha, though they do not extend to modifications of the poriferous zone, but are mainly changes in the actinostome and in the tuberculation. In fact, the group of Echinoconidæ seems to hold somewhat the same relation to the Clypeastroids which the Salenidæ hold to the Cidaridæ, and the earliest genus of the group Pygaster has remained, like Cidaris, a persistent type to the present day.

The earliest Clypeastroids are all forms which resemble the Fibularina and the genera following Echinocyamus and Fibularia; they are mainly characterised by the same changes which an Echinarachnius or a Mellita, for instance, undergoes as it passes from its Echinocyamus stage to the Laganum or Encope stage. The comparison is somewhat more complicated when we come to the Spatangoids. The comparison of the succession of genera in the different families, as traced in the Desmosticha and Clypeastroids, is made difficult from the persistency of the types preceding the Echinoneidæ and the Ananchytidæ, which have remained without important modifications from the time of the lower Cretaceous; previous to that time the modifications of the Cassidulidæ are found to agree with the changes which have been observed in the growth of Echinolampas. The early genera, like Pygurus, have many of the characteristics of the test of the young Echinolampas. The development of prominent bourrelets and of the floscelle and petals goes on side by side with that of genera in which the modification of the actinostome, of the test, and of the petals is far less rapid, one group retaining the Echinoneus features, the other culminating in the Echinolampas of the present day, and having likewise a persistent type, Echinobrissus, which has remained with its main structural features unchanged from the Jura to the present day. That is, we find genera of the Cassidulidæ which recall the early Echinoneus stage of Echinolampas, next the Caratomus stage, after which the floscelle, bourrelets, and petals of the group become more prominent features of the succeeding genera. Accompanying the persistent type Echinobrissus, genera appear in which either the bourrelets or petals have undergone modifications more extensive than those of the same parts in the genera of the Echinoneus or Caratomus type.

The earliest Spatangoids belong to the Dysasteridæ, apparently an aberrant group, but which, from the history of the young Hemiaster, we now know to be a strictly embryonic type, which, while it thus has affinities with the true Spatangoids, still retains features of the Cassidulidæ in the mode of development of the actinostome and of the petals, as well as of the anal system. The genera following this group, Holaster and Toxaster, can be well compared, the one to the young stages of Spatangus proper before the appearance of the petals, when the ambulacra are flush with the test, and when its test is more or less ovoid, the other to a somewhat more advanced stage, when the petals have made their appearance as semi-petals. In both cases the actinostome has the simple structure characteristic of all the young Spatangoids. The changes we notice in the genera which follow them lead in the one case through very slight modifications of the abactinal system, of the anterior and posterior extremities of the test, to the Ananchytid-like Spatangoids of the present day, the Pourtalesiæ, the genus Holaster itself persisting till well into the middle of the Tertiary period; while on the other side we readily recognise in the Spatanginæ which follow Toxaster (a persistent type which has continued as Palæostoma to the present day) the genera which correspond to the young stages of such Spatangoids as Spatangus and Brissopsis of the present day, genera which, on the one hand, lead from Hemiaster (itself still represented in the present epoch), through stages such as Cyclaster, Peripneustes, Brissus, and Schizaster, and, on the other, through Micraster and the like, to the Spatangoids, in which the development of the anal plastron and fasciole performs an important part, while in the former group the development of the peripetalous fasciole and of the lateral

[1] For this sketch of the embryology of the Petalosticha I have examined the young of Echinolampas, Echinoneus, Echinocardium, Brissopsis, Agassizia, Spatangus, Brissus, and Hemiaster.

fasciole can be followed. None of the genera of Petalosticha belonging to the other groups develop any fasciole in the sense of circumscribing a limited area of the test.

The comparison of the genera of Echini which have appeared since the Lias with the young stages of growth of the principal families of Echini, shows a most striking coincidence, amounting almost to identity, between the successive fossil genera and the various stages of growth. This identity can, however, not be traced exactly in the way in which it has usually been understood, while there undoubtedly exists in the genera which have appeared one after the other a gradual increase in certain families in the number of forms, and a constant approach in each succeeding formation, in the structure of the genera, to those of the present day. It is only in the accordance between some special points of structure of these genera and the young stages of the Echini of the present day that we can trace an agreement which, as we go further back in time, becomes more and more limited. We are either compelled to seek for the origin of many structural features in types of which we have no record, or else we must attempt to find them existing potentially in groups where we had as yet not succeeded in tracing them. The parallelism we have traced does not extend to the structure as a whole. What we find is the appearance among the fossil genera of certain structural features giving to the particular stages we are comparing their characteristic aspect. Thus, in the succession of the fossil genera, when a structural feature has once made its appearance, it may either remain as a persistent structure, or it may become gradually modified in the succeeding genera of the same family, or it may appear in another family, associated with other more marked structural features which completely overshadow it. Take, for instance, among the Desmosticha the modifications of the poriferous zone of the actinal and abactinal systems of the coronal plates, of the ambulacral and interambulacral systems, the changes in the relative proportion of the primary tubercles, and the development of the secondaries. These are all structural features which are modified independently one of the other; we may find simultaneous development of these features in parallel lines, but a very different degree of development of any special feature in separate families.

This is as plainly shown in the embryological as in the palæontological development. In the Cidaridæ there is the minimum of specialisation in these structural features. In the Diademopsidæ there is a greater range in the diversity of the structure of the poriferous zone and of the coronal plates, as well as of the actinal system. There is a still greater range among the Echinidæ, while among the Salenidæ the modifications, as compared to those of the Echinidæ and Diademopsidæ, are somewhat limited again, being restricted as far as relates to the poriferous zone and coronal plates, but specialised as far as the actinal system is concerned, and specially important with reference to the structure of the apical system. The special lines in which these modifications take place produce, of course, all possible combinations, yet they give us the key to the sudden appearance, as it were, of structural features of which the relationship must be sought in very distantly related groups. It is to this speciality in the palæontological development that we must trace, for instance, the Cidarid affinities of the Saleniæ, their papillæ, the existence of few large primary interambulacral tubercles, the structure of their apical system, and their large genital plates; while it is to their affinities with the Hemicidaridæ that we must refer the presence of the few larger primary ambulacral tubercles at the base of the ambulacral area, and by their Diademopsid and Echinidian affinities that we explain the indented imbricated actinal system with the presence of a few genuine miliaries. But all the structural features which characterise the earliest types of the Desmosticha can in reality be traced, only in a somewhat rudimentary form, even in the Cidaridæ. The slight undulation of the closely packed, nearly vertical poriferous zone is the forerunner of the poriferous zone first separated into vertical arcs and then into independent arcs. The limitation in the number of the rows of granules in the ambulacral zone, and their increase in size, is the first trace of the appearance of the somewhat larger primary ambulacral tubercles of the Hemicidaridæ and Saleniæ. The existence of the smooth cylindrical spines of the abactinal region of the test naturally leads to similar spines covering the whole test in the other families of the Desmosticha. The difference existing in the plates covering the actinal system from those of the coronal plates leads to the great distinction between the structure of the actinal system and of the coronal plates in some of the Echinidæ.

Passing to the Clypeastridæ and Petalosticha, we trace a parallelism of the same kind, and readily follow in the successive genera of fossil Clypeastroids, but often in widely separated genera, the precise modifications which the poriferous zone has undergone as it first becomes known to us in Echinocyamus and Fibularia, and as we find it in the most complicated petaloid stage of the Clypeastroids of the present day. We readily trace the changes the test undergoes from its comparatively ovoid and swollen shape to assume first that of the less gibbous forms, next that of the Laganidæ, and finally of the flat Scutellidæ; while we trace in the Echinanthidæ the persistent structural features of some of the earliest Clypeastroids, together with an excessive modification of the poriferous zone. Likewise for the Echinoconidæ we trace mainly the slight modifications of the poriferous zone and of the coronal plates, and finally, when we come to the Spatangidæ we find no difficulty in tracing from the most Desmostichoid of the Spatangoid genera the modifications of a test in which the ambulacral and interambulacral areas are made up of plates of nearly uniform size, in which the anterior and posterior extremities are barely specialised, to the most typical of the Ananchytidæ, in which the anterior and posterior extremities have developed the most opposite and extraordinary structural features. In a similar way we can trace among the fossil genera of different families the gradual development of the actinal plastron from its very earliest appearance as a modification of the posterior interambulacral area of the actinal side, or the growth of the posterior beak into an anal snout, the successive changes of the anal groove, the formation of the actinal labium, or the development of the bourrelets and phyllodes from a simple circular actinostome, the gradual deepening of the slight anterior groove of some early Spatangoid to form the deeply sunken actinal groove. Equally well we can trace the modifications of the ambulacral system as it passes from the simple poriferous zones of the earlier Spatangoids to genera in which the petaliferous portion makes its appearance, and finally becomes the specialised structure of our recent Spatangoid genera, such as Schizaster, Moira, and the like. Finally, we can trace to a certain extent the development of the fascioles on one side from genera like Hemiaster, in which the peripetalous fasciole is prominent, to genera like Brissopsis, Brissus, and the like, of the present day; on the other, perhaps, or both combined, the formation of a lateral and anal fasciole from genera like Micraster in Spatangus and Agassizia. Thus we must, on the same theory of the independent modifications of special structural features, trace the many and complicated affinities which so constantly strike us in making comparative studies, and which render it impossible for us to express the manifold affinities we notice, without taking up separately each special structure. Any attempt to take up a combination of characters, or a system of combinations, is sure to lead us to indefinite problems far beyond our power to grasp.

In the oldest fossil Clypeastroids and Petalosticha, as well as in the Desmosticha, we also find the potential expression of the greater number of the modifications subsequently carried out in genera of later date. The semipetaloid structure of some of the earlier genera of Spatangoids, the slight modifications of some of the plates of the actinal side near the actinostome, are the precursors, the one of the highly complicated petaloid ambulacra of the recent Spatangoids, the other of the actinal plastron, leading as it does also to the important differences subsequently developed in the anterior and posterior extremities of the test, as well as to the modifications which lead to the existence of a highly labiate actinostome. The appearance of a few miliaries near the actinostome constitutes the first rudimentary bourrelets.

Going back now to the Palæchinidæ, the earliest representatives of the Echini in palæozoic times, without any attempt to trace the descent of any special type from them, we may perhaps find some clue to the probable modifications of their principal structural features preparatory to their gradual disappearance. In the structure of the coronal plates, the specialisation of the actinal and abactinal systems, the conditions of the ambulacral system, we must compare them to stages in the embryonic development of our recent Echini with which we find no analogues in the fossil Echini of the Lias and the subsequent formations. In order to make our parallelism, we must go back to a stage in the embryonic history of the young Echini in which the distinction to be made between the ambulacral and interambulacral systems is very indefinite, in which the apical system is, it is true, specialised, but in which the actinal system remains practically a part

of the coronal system. But here the comparison ceases, and, although we can trace in the palæontological development of such types as Archæocidaris or Bothriocidaris modifications which would lead us without great difficulty, on the one side to the Cidaridæ, and on the other to the Echinothuriæ and Diadematidæ of the present day, we cannot fail to see most definite indications in some of the structural features of the Palæchinidæ of characteristics which we have been accustomed to associate with higher groups. The minute tuberculation, for instance, of the Clypeastroids and Spatangoids, already existing in the Melonitidæ, the genital ring, and anal system, are quite as much Echinid as Cidarid. The polyporus genera of the group represent to a certain extent the polypori of the regular Echini, and the lapping of the actinal plates of the Cidaridæ and of the coronal plates in some of the Diadematidæ, as well as the existence of such genera as Tetracidaris, of four interambulacral plates in Astropyga, and of a large number of ambulacral plates in some of the recent Echinometradæ, all these are Palæchinid characters which we can explain on the theory of the independent development of the structural features of which they are modifications. We should, however, remember, that the existence of a large number of coronal plates, especially interambulacral plates, in the Palæchinidæ, is a mere vegetative character, which they hold in common with all the Crinoids,—a character which is reduced to a minimum among the Holothurians, and still persists in full force among the Pentacrini of the present day, as well as the Astrophytidæ and Echinidæ.

It would lead me too far to institute the same comparison between the embryonic stages of the different orders of Echinoderms and their earliest fossil representatives. We may, however, in a very general way, state that we know the earliest embryonic stages of the order of Echinoderms of to-day, which, with the exception of the Blastoidea and Cystideans, are identical with the fossil orders, and that as far as we know they all begin at a stage where it would be impossible to distinguish a Sea-urchin from a Star-fish, or an Ophiuran, or a Crinoid, or an Holothurian,—a stage in which the test, calyx, abactinal and ambulacral systems are reduced to a minimum. From this identical origin there is developed at the present day, in a comparatively short period of time, either a Star-fish, a Sea-urchin, or a Crinoid; and if we have been able successfully to compare, in the development of typical structures, the embryonic stages of the young Echini with their development in the fossil genera, we may fairly assume that the same process is applicable when instituting the comparison within the different limits of the orders, but with the same restrictions. That is, if we wish to form some idea of the probable course of transformations which the earliest Echinoderms have undergone to lead us to those of the present day, we are justified in seeking for our earliest representatives of the orders such Echinoderms as resemble the early stages of our embryos, and in following, for them as for the Echini, the modifications of typical structures. These we shall have every reason to expect to find repeated in the fossils of later periods, and, going back a step further we may perhaps get an indefinite glimpse of that first Echinodermal stage which should combine the structural features common to all the earliest stages of our Echinoderm embryos.

And yet, among the fossil Echinoderms of the oldest periods, we have not as yet discovered this earliest type from which we could derive either the Star-fishes, Ophiurans, Sea-urchins, or Holothurians. With the exception of the latter, which we can leave out of the question at present, we find all the orders of Echinoderms appearing at the same time. But while this is the case, one of the groups attained in these earliest days a prominence which it gradually loses with the corresponding development of the Star-fishes, Ophiurans, and Sea-urchins, it has steadily declined in importance; it is a type of Crinoids, the Cystideans which culminated during Palæozoic times, and completely disappeared long before the present day. If we compare the early types of Cystideans to the typical embryonic Echinodermal type of the present day, we find they have a general resemblance, and that the Cystideans and Blastoids represent among the fossil Echinoderms the nearest approach we have yet discovered to this imaginary prototype of Echinoderms.

This may not seem a very satisfactory result to have attained. It certainly has been shown to be an impossibility to trace in the palæontological succession of the Echini anything like a sequence of genera. No direct filiation can be shown to exist, and yet the very existence of persistent types, not only among Echinoderms, but in every group of marine animals, genera which have continued to exist without interruption from the earliest epochs at which they occur to the present day, would prove conclusively that at any rate some groups among the marine animals of the present day are the direct descendants of those of the earliest geological periods. When we come to types which have not continued as long, but yet which have extended through two or three great periods, we must likewise accord to their latest representatives a direct descent from the older. The very fact that the ocean basins date back to the earliest geological periods, and have afforded to the marine animals the conditions most favourable to an unbroken continuity under slightly varying circumstances, probably accounts for the great range in time during which many genera of Echini have existed. If we examine the interlacing in the succession of the genera characteristic of later geological epochs, we find it an impossibility to deny their continuity from the time of the Lias to the present day. The Cidaris of the Lias and the Rhabdocidaris of the Jura are the ancestors of the Cidaris of to-day. The Saleniæ of the lower Chalk are those of the Saleniæ of to-day. Acrosalenia extends from the Lias to the lower Cretaceous, with a number of recent genera, which begin at the Eocene. The Pygaster of to-day dates back to the Lias; Echinocyamus and Fibularia commence with the Chalk. Pyrina extends from the lower Jura through the Eocene. The Echinobrissus of to-day dates back to the Jura. Holaster lived from the lower Chalk to the Miocene, and the Hemiaster of to-day cannot be distinguished from the Hemiaster of the lower Cretaceous.

Such descent we can trace, and trace as confidently as we trace a part of the population of North America of to-day as the descendants of some portion of the population of the beginning of this century. [But we can go no further with confidence, and bold indeed would he be who would attempt even in a single State to trace the genealogy of the inhabitants from those of ten years before. We had better acknowledge our inability to go beyond a certain point; anything beyond the general parallelism I have attempted to trace, which in no way invalidates the other proposition, we must recognise as hopeless.

But in spite of the limits which have been assigned to this general parallelism, it still remains an all-essential factor in elucidating the history of palæontological development, and its importance has but recently been fully appreciated. For, while the fossil remains may give us a strong presumptive evidence of the gradual passage of one type to another, we can only imagine this modification to take place by a process similar to that which brings about the modifications due to different stages of growth, —the former taking place in what may practically be considered as infinite time when compared to the short life history which has given us as it were a *résumé* of the palæontological development. We may well pause to reflect that in the two modes of development we find the same periods of rapid modifications occurring at certain stages of growth or of historic development, repeating in a different direction the same phases. Does it then pass the limits of analogy to assume that the changes we see taking place under our own eyes in a comparatively short space of time,—changes which extend from stages representing perhaps the original type of the group to their most complicated structures, —may, perhaps, in the larger field of palæontological development, not have required the infinite time we are in the habit of asking for them?

Palæontologists have not been slow in following out this suggestive track, and those who have been anatomists and embryologists besides have not only entered into most interesting speculations regarding the origin of certain groups, but they have carried on the process still further, and have given us genealogical trees where we may, in the twigs and branches and main limbs and trunk, trace the complete filiation of a group as we know it to-day, and as it must theoretically have existed at various times to its very beginning. While we cannot but admire the boldness and ingenuity of these speculations upon genetic connection so recklessly launched during the last fifteen years, we find that with but few exceptions there is little to recommend in reconstructions which shoot so wide of the facts as far as they are known, and seem so readily to ignore them. The moment we leave out of sight the actual succession of the fossils and the ascertainable facts of postembryonic development, to reconstruct our genealogy, we are building in the air. Ordinarily, the twigs of any genealogical tree have only a semblance of truth; they lead us to branchlets having but a slight trace of probability, to branches where the imagination plays an important part, to main

limbs where it is finally allowed full play, in order to solve with the trunk, to the satisfaction of the writer at least, the riddle of the origin of the group. It seems hardly credible that a school which boasts for its very creed a belief in nothing which is not warranted by common sense should descend to such trifling.

The time for genealogical trees is passed; its futility can, perhaps, best be shown by a simple calculation, which will point out at a glance what these scientific arboriculturists are attempting. Let us take, for instance, the ten most characteristic features of Echini. The number of possible combinations which can be produced from them is so great that it would take no less than twenty years, at the rate of one new combination a minute for ten hours a day, to pass them in review. Remembering now that each one of these points of structure is itself undergoing constant modifications, we may get some idea of the nature of the problem we are attempting to solve, when seeking to trace the genealogy as understood by the makers of genealogical trees. On the other hand, in spite of the millions of possible combinations which these ten characters may assume when affecting not simply a single combination, but all the combinations which might arise from their extending over several hundred species, we yet find that the combinations which actually exist—those which leave their traces as fossils—fall immensely short of the possible nnmber. We have, as I have stated, not more than twenty-three hundred species actually representing for the Echini the results of these endless combinations. Is it astonishing, therefore, that we should fail to discover the sequence of the genera, even if the genera, as is so often the case, represent, as it were, fixed embryonic stages of some Sea-urchin of the present day? In fact, does not the very history of the fossils themselves show that we cannot expect this? Each fossil species, during its development, must have passed through stages analogous to those gone through by the Echini of the present day. Each one of these stages at every moment represents one of the possible combinations, and those which are actually preserved correspond only to the particular period and the special combination which any Sea-urchin has reached. These stages are the true missing links, which we can no more expect to find preserved than we can expect to find a record of the actual embryonic development of the species of the present day without direct observation at the time. The actual number of species in any one group must always fall far short of the possible number, and for this reason it is out of the question for us to attempt the solution of the problem of derivation, or to hope for any solution beyond one within the most indefinite limits of correctness. If, when we take one of the most limited of the groups of the animal kingdom, we find ourselves engaged in a hopeless task, what must be the prospect should we attack the problem of other classes or groups of the animal kingdom, where the species run into the thousands, while they number only tens in the case we have attempted to follow out? Shall we say "ignorabimus" or "impavidi progrediamus" valiantly to chase a phantom we can never hope to seize?

NOTES

THE Second International Geological Congress will be held at Bologna in September, 1881. It is proposed to award a prize of 5,000 francs for the best international scale of colours and conventional signs for the graphic representation of formations on geological maps and sections. Each scale should be accompanied by an explanatory memoir and a sufficient number of maps and sections relative to regions of different geological characters; for the memoirs the French language is recommended. The names of the competitors should be inclosed in sealed envelopes, on which should be a motto. The scales and memoirs should be addressed, before the end of May, 1881, to the President of the Committee, Signor J. Capellini, 65, Via Zamboni, Bologna.

"THE Official Guide and Handbook to Swansea and its District," prepared at the request of the Local Committee by Mr. S. C. Gamwell, is a really useful little work, which must prove of permanent value as a guide to Swansea. It contains much carefully compiled information on the History and Antiquities of Swansea, its literary and scientific, educational and other institutions, industries, places of interest in and around the town, geology of the district, palæontology, and natural history. A considerable amount of space is appropriately devoted to the scientific aspects of the district, the information given is very full, and we believe trustworthy. An excellent map is prefixed, and the work as a whole is creditable to the Local Committee and to the author.

ON Sunday a statue of Denis Papin was unveiled at Blois, where he was born in 1647.

THE late Mungo Ponton, W.S., Fellow of the Royal Society of Edinburgh, whose death was recently announced, was known as the discoverer of the peculiar effect of light on gelatine when treated with the bichromates, which was afterwards practically applied in the autotype process. He obtained the silver medal from the Royal Society of Edinburgh in 1838 for "model and description of improved electric telegraph." He was the first who employed the photographic method for registering automatically the fluctuations in thermometers and other instruments, and for this service he received also the silver medal of the same society in 1845. Several papers of his appeared in the new *Philosophical Journal* and in the *Quarterly Journal of Science.*

THE Fifth Annual General Meeting of the Mineralogical Society of Great Britain and Ireland was held at Swansea on August 27, Mr. Jas. S. Merry, F.C.S., in the chair. A favourable report was presented by the Council and adopted by the meeting. The election of the following new members was announced:—Dr. Jas. Hector, F.R.S., of New Zealand, Mr. Thos. Stewart of Glasgow, Rev. R. Graham, LL.D., of Errol, Perthshire; Mr. Jos. Gill of Leadhills, and Rev. Geo. Gordon, LL.D., of Elgin. The following papers were read and discussed:—"On the Chemical Formula of Epidote," by M. l'Abbé Renard; "On Certain Crystallised Furnace-products," by Wm. Terrill, F.C.S.; "On the Serpentine and Hornblendic, and Schistose Rocks of Porthalla in Cornwall," by H. Collins.

ON the evening of July 20, about half-past eight o'clock, a remarkable meteor, said to have resembled a comet, apparently about twenty yards in length, was observed at Vizimgaum and other places in India, traversing the sky from south to north, and remaining visible for about three-quarters of a minute, during which time the whole sky and country were brilliantly illuminated. The meteor then burst, and some time afterwards a loud sound like distant thunder, which lasted two minutes, was heard.

THE rainfall in Southern China appears to have been abnormally large in the early part of this summer, for we learn from the *Daily Press* of Hongkong that the rainfall in that colony during the month of June was no less than 28·06 inches, compared with 11·32 inches in June, 1879, 15·36 inches in June, 1878, and 9·37 inches in June, 1877. It is stated that so large a rainfall as we have mentioned has never before been registered in Hongkong.

THE Government printer at Brisbane has published three valuable Reports, by Mr. Robert L. Jack, the Geological Surveyor of Northern Queensland, who during the last few years has done excellent service in the cause of both geography and geology in Cape York Peninsula. The first of these Reports deals with the geology and mineral resources of the district between Charters Towers gold-fields and the coast, and is illustrated with a map and several woodcuts taken from photographs, while the second is a preliminary Report on the geological features of part of the coast range between the Dalrymple and Charters Towers roads. The third Report treats of the important Bowen River coal-field. This is accompanied by a map and some large and interesting woodcuts.

At Caràcas in Venezuela on August 1 there was a heavy shock of earthquake at 7 p.m.

The United States Government proposes to hold an International Sanitary Congress at Washington in January, 1881.

The Rev. A. E. Eaton has begun a series of Notes on the Entomology of Portugal in the *Entomologist's Monthly Magazine*.

In No. 9, vol. xxxviii. of *Globus* an account of Dr. Potagos's travels in the regions of the Nile and Welle is given.

Among the articles in No. 2 of the *American Journal of Philology* (Macmillan and Co.) is one of considerable scientific interest—"Recent Investigations of Grimm's Law," by Mr. H. C. G. Brandt of Johns Hopkins University.

We are requested by Lieut. Temple to publish the following letter addressed to him by Lieut. Col. Fr. Sejersted, Director of the Royal Norwegian Geographical Survey Office:—"Christiania, August 7, 1880.—Sir,—You may possibly have noticed that I have replied in some English newspapers to remarks stated in those papers to have been made by you at a meeting on the 19th of May last of the Society of Arts, with respect to our Norwegian coast charts, and that I have especially alleged that your remarks must have been caused by ignorance of the present stage of our coast charts. I now learn from a copy of the *Journal* of the Society of Arts, containing your paper complete, that the said newspaper statements were misguiding extracts, and that your censure was not pointed against our coast charts, but against an English belabouring of those charts in no way connected with the geographical survey of Norway. As a matter of course my reply would not have been published if the said newspaper statements had been correct concerning the main point to us, and I hereby declare with great pleasure that the copy of the *Journal* of the Society of Arts, containing your paper, proves plainly that you are intimately acquainted with our coast charts and coast descriptions, which are mentioned by you in a very satisfactory manner to us. I express my wish that these lines may help to make good the injustice you have suffered from the misconceptions caused by the ambiguous newspaper statements, and I beg to leave it to you to use this letter at pleasure."

The additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. Geo. G. Turner; two American Moorhens (*Gallinula galeata*) from America, presented by Mr. G. H. Hawtayne, C.M.Z.S.; a Turnstone (*Strepsilas interpres*), captured at sea, off the Azores, presented by Capt. A. McRitchie, s.s. *Utopia*; two Koodoos (*Strepsiceros kudu*) from Africa, a Harnessed Antelope (*Tragelaphus scriptus*) from Gambia, a Syrian Fennec Fox (*Canis famelicus*) from North Africa, a King Vulture (*Gypagus papa*) from Tropical America, deposited; a Nylghaie (*Boselaphus pictus*) from India, a Michie's Tufted Deer (*Elaphodus michianus*) from China, a Cuvier's Toucan (*Ramphastos cuvieri*) from the Upper Amazons, purchased.

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, August 16.—M. Wurtz in the chair.—The following papers were read:—Summary report of the cruise of *Le Travailleur* (continued), by M. Alph. Milne-Edwards. He describes the various animal species obtained.—On the establishment of hospital stations in Equatorial Africa, by M. de Lesseps. This reports the progress of Capt. Bloyet in the East (who reached Usagara on July 2) and M. de Brazza in the West (who is seeking a suitable position on one of the affluents of the Ogowé).—On the embryos accompanying cysticersi in pork, by M. Poincaré. Pork may contain microscopic germs of Tænia which may quite escape ordinary inspection. Raw meat of any kind should be avoided.—On some formulæ relative to hypergeometric functions of two variables, by M. Appell.—On various attempts at demonstration of the theorem of Fermat, by M. Pépin.—Observation on a group of lines in the solar spectrum, by M. Thollon. With the centre of the solar image, on his apparatus (on a mountain near Nice) he notes four lines, *a*, *b*, *c*, *d*, of which *a* and *b* are close to each other, and similarly *c* and *d*; *b* and *c* are of iron, *a* and *d* are telluric. On directing the apparatus to the two ends of the equatorial diameter, the iron lines are displaced relatively to the others, conformably to theory.—On the cause of variations of fixed points in thermometers, by M. Crafts. He describes experiments which reduce to *nil*, or a very small amount, the *rôle* of pressure in permanent elevation of the zero point. Glass blown at the lamp and long exposed to heat diminishes in volume through interior work.—On rapid alcoholic fermentation, by M. Boussingault. This relates to fermentation in a liquid that is boiling under a pressure so weak that the heat does not alter the organism of the ferment, while yet it is sufficient to expel the alcohol and the carbonic acid. Glycerine appears during this rapid fermentation.—Spectral examination of thulium, by M. Thalén.—On the absorption-spectra of metals forming part of the groups of yttria and of cerite, by M. Soret.—On erbine, by M. Clève. The atomic weight of the metal he finds to be 166 (ytterbium 173).—Measurement of the intensity of some dark lines of the solar spectrum, by M. Gouy. His method shows clearly the telluric nature of the group B (between 6866 and 6880), by reason of their greater intensity.—On polar electricity in hemihedric crystals with inclined faces, by MM. Jacques and Curie. They show that in all the non-conducting substances studied the direction of the electric poles is connected with the position of the hemihedric facettes. M. Thenard recalled experiments bearing on the subject made by his son fifteen years ago.—New results of utilisation of solar heat obtained at Paris, by M. Pifre. He succeeds in utilising 80 per cent. of the solar heat as against 50 (Mouchot). The reflector is made of three truncated cones, so that the generating line is a broken one. The focus is thus concentrated in much less length, and the height of the boiler may be diminished one half (without increasing its diameter). When the sky is clear the boiling of fifty litres is obtained in less than forty minutes, and the pressure rises 1 atm. every seven or eight minutes. The steam-engine is specially adapted for solar receivers.—Production of crystals of sesquichloride of chromium of persistent green colour, by M. Mengeot.—On the inconveniences presented, with regard to physiological reactions, in cases of poisoning with morphine, by the substitution of amylic alcohol for ether in the process of Stas, by MM. Bergeron and L'Hote.—On the experiment of the great cervical sympathetic, by MM. Dastre and Morat. They demonstrate the existence of vaso-dilator as well as vaso-constrictor nerves in the cervical sympathetic.—Morphological signification of the appendices serving for suspension of chrysalides, by M. Künckel. They are (in Lepidoptera) hooks of membranous anal legs modified and adapted to special biological conditions.—On a new station of the age of stone at Hanaweh, near Tyre (Syria), by M. Lortet. Myriads of flints (of very primitive form), along with numerous fragments of bone and teeth, were found in a kind of conglomerate or breccia.—On the falling stars of August 9, 10, and 11, 1880, by M. Chapelas. The mean horary number is only 53·7, making a difference of 69·3 with that last year. This seems to limit the return of the maximum of August between 1848 and 1879, giving a period of thirty-two or thirty-three years, quite like that of the phenomenon of November 12 and 13.

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